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Abstract

This dissertation is composed of three essays: two in the field of labour economics and one in political economics. The first essay studies the role of growing workplace heterogeneity for the stagnation of the gender pay gap on the West German labour market during the 1990s and 2000s. The analysis shows that the expansion of workplace-specific wage premiums over that time period prevented the gender wage gap from narrowing by around 15% or 3.6 log points, thus offering a potential explanation for the emerging puzzle of stalling gender wage inequalities in spite of continuously converging skill supplies. This effect is not driven by a re-sorting of men and women across high and low wage firms, but is entirely attributable to a widening of the distribution of wage premiums which benefited men (who were more likely to work at higher wage firms) relatively more than women. The study further shows that rising wage flexibilisation, facilitated by deunionisation and decentralisation tendencies within unions, has led to higher rent-sharing elasticities, and thereby catalysed the role of workplace heterogeneity for overall inequality and the wage gap between genders.

The second essay investigates the impact of a refugee-driven labour supply shock on native wages and employment. By exploiting a large and unexpected refugee wave hitting the West German labour market between 1988 and 1993, the analysis shows that immigration has a short run negative impact on native wages and employment: an increase in local immigrant employment by 1% reduces native wages and employment by about 0.68 and 1.13%, respectively; in the longer perspective, however, these negative effects disappear. The study also shows that about two-thirds of the local employment decline is compensated by corresponding employment gains in regions not affected by immigration. Both findings — the difference between short and long run effects and the redistribution of native employment across regions — are important for the political evaluation of immigration.

The third essay investigates the political determinants of municipality amalgamations. By exploiting a boundary reform in the state of Brandenburg, which reduced the number of municipalities by about 70%, the study asks whether party representation in the town council influences the structure of municipality mergers by affecting the political decision makers' probability to remain in power. The empirical analysis exploits two counterfactual contrasts: first, voluntary to forced mergers; and second, voluntary to simulated mergers. The empirical estimates suggest that political representation matters for the structure of mergers that materialise.

Keywords:
labour economics, political economics, gender wage gap, firm-specific wage premiums, collective bargaining, childbirth, rent sharing, administrative data, linked employer-employee data, immigration, labour supply shock, refugees, mergers, political determinants, municipality mergers

Zusammenfassung

Die vorliegende Dissertation setzt sich aus drei Aufsätzen zusammen: zwei im Bereich der Arbeitsmarktökonomie und einer im Bereich der politischen Ökonomie. Der erste Aufsatz untersucht die Rolle der zunehmenden Firmenheterogenität für die Stagnation des Gender Wage Gaps — der Lohnlücke zwischen Männern und Frauen — auf dem westdeutschen Arbeitsmarkt in den 1990er und 2000er Jahren. Die Ergebnisse zeigen, dass die deutliche Zunahme der Heterogenität in den firmenspezifischen Löhnen während dieses Zeitraums einen Rückgang des Gender Wage Gaps um 15% bzw. 3,6 Log-Prozentpunkte verhindert hat. Somit stellt die wachsende Firmenheterogenität eine potenzielle Erklärung für die Stagnation des Gender Wage Gaps in Zeiten von weiterhin abnehmenden Unterschieden im Humankapital von Männern und Frauen dar. Dieser Effekt ist nicht etwa auf eine Veränderung der Verteilung von Männern und Frauen auf Hoch- und Niedriglohnbetriebe zurückzuführen ("re-sorting"), sondern vielmehr auf eine wachsende Ungleichheit in den firmen-spezifischen Lohnkomponenten, von der Männer aufgrund ihrer höheren Wahrscheinlichkeit in Hochlohnbetrieben zu arbeiten stärker profitiert haben als Frauen. Darüber hinaus zeigen die Analysen, dass eine zunehmende Lohnflexibilisierung, bedingt durch einen Rückgang der Tarifbindung einerseits und wachsende Dezentralisierungs- und Flexibilisierungstendenzen innerhalb der vorhandenen Tarifbindungsregime andererseits, den Anstieg der Lohnungleichheit zwischen Betrieben und folglich die Lohnungleichheit zwischen Männern und Frauen verstärkt hat.

Der zweite Aufsatz untersucht die Auswirkungen des Anfang der 1990er Jahre von Flüchtlingsmigranten verursachten, plötzlichen Anstiegs des Arbeitskräfteangebots auf Löhne und Beschäftigung der einheimischen Arbeitnehmer. Die empirischen Analysen zeigen, dass ein durch Zuwanderung induzierter Anstieg des Arbeitskräfteangebots kurzfristig negative Auswirkungen auf das Lohn- und Beschäftigungswachstum der einheimischen Arbeitskräfte hat. So geht ein 1%iger Zuwachs in der Beschäftigung von Migranten mit einer Reduzierung des lokalen Lohn- und Beschäftigungswachstums in den betroffenen Regionen um durchschnittlich etwa 0,68 bzw. 1,13% einher; auf längere Sicht zeigen sich indes keine negativen Auswirkungen. Weitergehend dokumentiert die Studie, dass etwa zwei Drittel des lokalen Beschäftigungsrückgangs durch entsprechende Beschäftigungsgewinne in solchen Regionen kompensiert werden, die von der Flüchtlingszuwanderung nicht betroffen sind. Beide Aspekte — die Unterschiede zwischen kurz- und langfristigen Konsequenzen sowie die Umverteilung der Beschäftigung zwischen Regionen — sind für die politische Evaluation der Vor- und Nachteile von Migration von Bedeutung.

Der dritte Aufsatz untersucht die politischen Determinanten von Gemeindezusammenlegungen auf der Grundlage einer umfassenden Gemeindegebietsreform im Bundesland Brandenburg. Die Studie geht der Frage nach, ob die Parteienlandschaft im Gemeinderat einen Effekt auf die Struktur von Gemeindezusammenlegungen hat, indem sie die Wahrscheinlichkeit der Wiederwahl und folglich des Machterhalts der im Amt befindlichen politischen Entscheidungsträger beeinflusst. Zur Beantwortung dieser Frage nutzt die empirische Analyse zwei unterschiedliche Ansätze: zum einen werden die Parteienstrukturen zwischen freiwilligen und erzwungenen Gemeindezusammenlegun-

gen verglichen; zum anderen wird der gleiche Zusammenhang zwischen freiwilligen und simulierten Fusionen untersucht. Die empirischen Ergebnisse deuten darauf hin, dass die Parteienstruktur für die Realisierung von Gemeindezusammenlegungen von Bedeutung ist.

Schlagwörter:
Arbeitsmarktökonomie, politische Ökonomie, Geschlechterlohndifferenzial, betriebsspezifische Lohnprämien, Gewerkschaften, Kindergeburten, Sozialversicherungsdaten, verknüpfte Betriebs- und Personendaten, Einwanderung, Arbeitsangebot, Flüchtlinge, Gemeindezusammenlegungen

To Mona

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1 Introduction

Over the past decades, women have made tremendous strides in the labour market across many advanced economies. They have continuously narrowed the labour force participation gap and the divergence in effective labour market experience over the lifecycle. They have reduced and sometimes even reversed the education gap, and they have lowered occupational and industrial segregation. The relative wage of women has long been a banner for how the society and the labour market values women's work relative to men's, and it still represents the figurehead of gender (in-)equality in advanced labour markets today. Indeed, for many years, the pervasive progression of women along the various dimensions of gender inequality has been deeply interwoven with a sustained closing of the wage differential between gender. But not so any more in recent years.

Beginning in the 1990s, the evolution of the wage gap between men and women decoupled from the trends in several classic indicators of gender convergence. While women continued their successful pursuit for greater equality in terms of economic opportunity and participation, family division of labour, lifetime experience, and educational attainment, their relative wage plateaued, with equality still a long way off. To provide context, some descriptive macro evidence: as of 2015, women's gross hourly wages in the European Union (EU-28) were about 16.4% below men's (Eurostat). Germany, the largest economy in the EU-28, placed 25th in that statistic, featuring an average male-female wage gap of 21.6%, which is topped only by the Czech Republic, Austria, and Estonia. These figures have moved little since the mid 1990s. For example, between 1995 and 1999, the average gender pay gap in Germany was 21%. This observation also puts into perspective the decline from 22 to 21%, which the Federal Statistical Office reported for 2016. Perhaps even more alarming than the stalling of women's progress, however, seems to be the worsening of their future economic prospects that is becoming apparent in some recent numbers on gender equality. For example, while the OECD Employment Outlook found a 14% wage gap for Germany in 2012, the most recent report based on 2015 data reveals a rise to 17% (OECD, 2014, 2017). Of course, such cross-country and macro-level comparisons suffer many limitations, among other things, different regulatory frameworks that alter participation incentives and, consequently, what types of women are observed on the labour market. Yet what these statistics do show at the minimum is that, although women today 'look' more like men than ever before (and continue to make headway in many key dimensions), gender parity in wages is still a long way off — and the reason for this decoupling remains an open question.

For decades, economists have proposed and tested explanations for gender inequality. The traditional causes, in the spirit of Becker (1971) and Mincer (1974), include differences in basic human capital (education and work experience), the division of market work, and (statistical) discrimination. However, in view of considerable convergence and in some cases (like education) overtaking by women over the past years, these classic explanatory factors have forfeited their long-standing dominant role and by now account for only a small proportion of the gender pay gap (Blau and Kahn, 2017). Yet other explanations such as segregation across occupations, industries and workplaces (Blau, 1977; Groshen, 1991; Carrington and Troske, 1998; Bayard et al., 2003; Card et al., 2016), work interruptions due to childbirth and childrearing (Angelov et al., 2016; Kleven et al., 2017), as well as compensating differentials for fewer working hours and temporal flexibility (Goldin, 2014) continue to have salience, and are often gaining importance. All of these dimensions represent, in some sense or the other, *gender-specific* determinants of the wage gap (Blau and Kahn, 1997). But there is also a more fundamental effect on the gender pay gap emerging from changes in the *wage structure*, i.e., the returns to individual skills (both observed and unobserved) and the remuneration received for working in certain firms or in particular sectors of the labour market. For example, as noted in Blau and Kahn (1997) "since women on average have less experience than men, an increase in the return to experience (as, in fact, occurred during the 1980s [in the US]) would cause the gender pay gap to widen, even if women's *relative* level of experience and their gender-specific treatment by employers remained the same." This is just another way of saying that US women in the 1980s had been 'swimming upstream' against the tide of a labour market growing increasingly unfavourable to less experienced workers. Clearly, this reasoning is by no means limited to experience. Rather, it applies more generally to any differences in 'endowments' between men and women, including, in particular, the type of workplace. This insight is pivotal because the stalling gender pay gap is not the only common trend across advanced labour markets over the past decades; a second being a dramatic expansion of overall wage inequality, which, in the case of Germany, took off in the mid 1990s (Dustmann et al., 2009), i.e., in coincident timing with the stagnation of gender inequality. Looking deeper into the drivers of this widening, the one thing that stands out prominently, not only in Germany but also in other countries, is a substantial expansion in workplace-specific wage premiums (Card et al., 2013, 2016; Macis and Schivardi, 2016; Song et al., 2015; Skans et al., 2008). Following Blau and Kahn (1997)'s metaphor of 'swimming upstream', this expansion in the firm-specific component of wages provides a simple explanation for why the relative wage of men and women no longer converged in recent years: if women are employed in firms that pay lower average wages or if they tend to earn lower wages at the same firms as men, then they are slipping down the wage distribution as the dispersion of workplace-specific wages increases. Put another way, the salient expansion of workplace-specific wage premiums might conceal what would otherwise have been a convergence of the pay gap between genders.

This mechanism is at the core of chapter 2 of this dissertation. Building on a rich panel of firms matched with longitudinal information on workers for the West German labour market between 1995 and 2008, the analysis scrutinises the role of growing workplace heterogeneity in recent trends of the gender wage gap. The study establishes three main findings: first, it shows that, roughly within a decade, the contribution of firm-specific pay premiums to the gender wage gap rose by about 15% or 3.6 log points, up from an initial 11% share in the 24.7 log points gender gap during the 1990s, to a 26% share of the same gap in the 2000s. Phrased differently, firms matter, and they increasingly do so in more recent years. Second, using simple counterfactual analyses, the study provides evidence that the growing contribution of firm premiums sources in two complementary mechanisms: i) firms with initially higher wages (where men are concentrated) feature higher growth rates in their premiums than firms with initially lower wages (where women are concentrated); and ii) male firm premiums grow faster than female premiums in the same firms. Crucially, this means that neither men nor women work at systematically different types of firms in the 2000s than they did in the 1990s. Rather, women were more likely to work at lower wage firms to begin with, and these firms subsequently experienced a slower wage growth (especially for women) than firms with initially higher wages. Hence, Blau and Kahn (1997)'s figurative comment of women 'swimming upstream' in a labour market growing more and more unfavourable to women remains true today, but with an important modification: rather than growing returns to skills and experience working to the detriment of women, it is the rapidly growing wedge in wages between the most and least productive firms in the economy that put women at a relative disadvantage. Finally, the analysis connects the growth in workplace heterogeneity to changes in the institutional framework that influence the wage setting process. Focusing on collective bargaining, the study shows that rent-sharing elasticities rose dramatically inside the covered sector of the labour market, suggesting that growing wage flexibilisation on the part of unions enabled a closer alignment between changes in firm level productivity and wages, and this put women, who were already working at less productive firms to begin with, at a relative disadvantage. Moreover, the analysis shows that the process of deunionisation itself also contributed to the growing firm-related gender wage gap by releasing the upward pressure on female wages in formerly covered firms.

Chapter 3 of this dissertation revisits another traditional question in labour economics: the impact of an immigrant-induced labour supply shock on native wages and employment. By exploiting a large and unexpected inflow of refugee migrants into the West German labour market between 1988 and 1993, the analysis sheds some new light on the short and long run effects of immigration on native wages and employment at the level of local labour markets. Based on detailed administrative data and distance from the south and east German border as instrumental variables, the analysis presents three main findings. First, it highlights the importance of distinguishing between the immediate (short run) consequences of immigration, and the lagged (long run) effects that arise in the process of local economies adjusting to the additional supply of labour. Specifically, the empirical estimates suggest that, within five years, local immigrant inflows reduce native wages and employment on average by about 0.68 and 1.13%, respectively, but seem to have no or even positive effects in the longer perspective. The employment effects are more pronounced among unskilled than skilled workers, and stronger for workers above age 30 than labour market entrants, while wage reductions are concentrated among skilled and middle-aged workers. Second, by following individual workers across regions and employment states, the analysis shows that the economy-wide employment decline is substantially smaller than suggested by the local elasticities. In particular, it shows that about two-thirds of native workers who leave or no longer enter employment in affected regions immediately find employment in other, unaffected areas. Taken at face value, immigrants enhance cross-regional mobility, and thus induce a reshuffling of existing employment relationships, whereas overall nonemployment rates increase only moderately. Third, by decomposing each margin of employment response into inflows and outflows, the study further documents that a reduction of inflows accounts for the predominant portion of the local employment loss, contributing about two-thirds of the entire decline. This means that outsiders carry most of the burden of immigration, and thus shield native incumbent workers from the adverse short run effects of immigration. Altogether, the analysis in chapter 3 suggests that a better understanding of the dynamic adjustments that help local economies to absorb and capitalise on immigrant labour in the short and long run is an important concern for future research in the field of immigration economics.

The last chapter turns to another field in economics: the political economy of municipality mergers. Administrative boundaries influence economic operation, and their definition pervades, among other things, the trade and immigration literature — in fact, several analyses in chapter 3 draw on one such definition (districts) as a simple means of delineating local labour markets. But what factors determine these boundaries? Basic theoretical considerations in the spirit of Oates (1968) suggest that the optimal size of local, self-contained administrative entities is a function of better local representation on the one hand, and cost efficiency in the provision of public goods on the other hand. Yet a more recent literature has recognised the role of political decision makers in the process of defining local units which constitute the politicians direct sphere of control (Saarimaa and Tukiainen, 2014; Hyytinen et al., 2014). Building on the idea that political decision makers strive to maintain their power in newly formed administrative units, the analysis in chapter 4 aims to answer the question whether and to what extent local party representation in the town council — as a measure of the probability to stay in office — influences the merger decision. To estimate the effect, the analysis exploits a large boundary reform in the German state of Brandenburg which reduced the number of municipalities from 1,489 to 421, coupled with rich fiscal and socio-economic information on individual municipalities. To identify the role of political congruence in merger decisions, the study exploits two alternative strategies: first, it takes advantage of the particular design of the reform which first allowed municipalities to merge voluntarily before the state forced the remaining municipalities that did not meet the set targets to merge with arbitrary partners; second, it contrasts voluntary mergers with a set of simulated mergers constructed from the initial map of municipalities. Overall, both approaches indicate that the dominant party share in merger partners' town councils positively affects the probability of merger formation; that is, political congruence indeed influences the merger decision, indicating that the often invoked assumption that politicians act in the best interest of their constituents, or simply represent the median voter's position might be violated. Hence, understanding the particular incentive structure of the political actors may be important for the design of boundary reforms as the objective function of the decision makers might differ (perhaps strongly) from the initial intentions of the reform.

All three subsequent chapters are self-contained and can be read independently. Chapter 2 is single-authored and currently under review (Revise and Resubmit) for the *American Economic Journal: Applied Economics*. The version that is included in this dissertation corresponds to the draft that I resubmitted to the journal on September 13th, 2017. Chapter 3 has been coauthored with Kai Priesack, and chapter 4 is joint work with Ronny Freier (my second supervisor) and Abel Schumann.

2 Assessing the Role of Workplace Heterogeneity in Recent Trends of the Gender Wage Gap

2.1 Introduction

Following remarkable convergence of gender pay over the last century (Goldin, 2014), wage differentials between men and women stagnated across many advanced labour markets, with substantial inequalities remaining (Blau and Kahn, 2017). This stagnation has occurred against a continuous narrowing of skill supplies, and persists within detailed industries and occupations. Using administrative data for the West German labour market in 1995-2008, I propose a simple explanation for this puzzle: a salient expansion of firm-specific wage premiums adversely affected women by widening the wage spread between more and less productive firms and by increasing pay gaps within these firms.[1]

That firm-specific wages matter for gender inequality through segregation along the firm quality space and/or different negotiation skills is a widely documented fact.[2] The first comprehensive evidence of these two channels is provided by Card, Cardoso, and Kline (2016) [CCK] in a recent study for Portugal. Based on a simple rent-sharing model linking firm-specific wage premiums to average firm productivity, they demonstrate how gender disparities in firm premiums can be decomposed into one effect that measures differences in the employment shares between higher and lower wage firms (sorting), and another effect that quantifies gender differences in wages paid by the same firm (bargaining).[3] CCK show that firm premium inequality accounts for about 20% of the overall gender gap, with two thirds of this explained by the sorting channel, and one third attributable to the bargaining channel.

In this paper, I extend the analysis of CCK to investigate how *changes* in the gender-specific distributions of firm premiums affect the *trends* in gender wage differentials. This

[1] This mechanism is related to studies by Blau and Kahn (1995, 1996, 1997) who show that overall wage inequality is an important source of gender inequality across countries. A useful trope for the patterns described in the present research is the notion of "swimming upstream" against changes in gender-specific returns to working for lower and higher wage firms.

[2] The segregation channel is emphasised in Blau (1977), Groshen (1991), Carrington and Troske (1998), Bayard et al. (2003), and Achatz et al. (2004). Differential negotiation skills are highlighted in Babcock and Laschever (2003), Bowles et al. (2005), Bowles et al. (2007), Bertrand (2011), and Blau and Kahn (2017).

[3] The first component is often referred to as coefficient effect, and it measures the *returns* to working for a firm. The second component is known as endowment effect, evaluating the distributions of men and women at different types of firms.

relationship is determined by two complementary mechanisms: first, changes in the relative distributions of male and female employment across firms – a relocation channel; and, second, changes in the firm-specific premiums paid to each gender – a wage structure channel. My analysis focuses on this second channel, enabling me to interpret changes in the sorting and bargaining effects from the CCK decompositions as follows: a rise in the sorting component implies that firms with initially higher wage premiums (where men are more concentrated) feature higher firm premium growth rates over time; and a rise in the bargaining component suggests that male firm premiums grow faster than female firm premiums at the same firms.

The West German labour market represents a useful test case to study the relationship between firm-specific wage premiums and gender pay gaps over time. First, despite continuous convergence in participation, human capital, and career prospects, the gender gap in full-time daily wages stagnated at a level well above 20% (Antonczyk et al., 2010). Second, wage inequality expanded rapidly since the 1990s, and this expansion was dominated by rising workplace heterogeneity (Card, Heining, and Kline, 2013) [CHK].[4] As I document below, this development is highly correlated with a parallel rise in the dispersion of firm productivity. Third, the country witnessed dramatic changes in the functioning of its labour market: since the early 1990s, the system of industrial relations underwent an unprecedented decentralisation, with union coverage falling sharply and opening clauses permeating (Dustmann et al., 2014). This trend was accompanied by a remarkable rise in domestic outsourcing (Goldschmidt and Schmieder, 2017) and extensive labour market reforms that accommodated a growing flexibilisation of wages and employment (CHK). Unions influence the wage setting behaviour of firms, with corresponding ramifications for overall wage inequality. This paper investigates their impact on the trends in gender-specific firm premiums, and provides a direct link between the potential drivers of overall wage inequality and gender differentials in firm-specific pay. Taken together, these three developments share many structural similarities with other European and Anglo-Saxon countries, suggesting that the German case may hold lessons for the broader international context.[5]

To analyse the impact of growing workplace heterogeneity, and to differentiate between changes in sorting and bargaining, I split the 1995-2008 period into two overlapping intervals, 1995-2001 and 2001-2008, and estimate separate linear models with additive worker and firm effects — as in Abowd, Kramarz, and Margolis (1999) [AKM] — for each gender.[6] The firm effects in these regressions represent an employer's *gender-specific* pay policy that is cleaned from unobserved worker ability and returns to education/experience, and I show that they are highly correlated across genders. Relating the estimated firm premiums to measures of firm productivity, I find that rent-sharing elasticities grow rapidly over time, suggesting a

[4] Similar results have been documented for France (Abowd et al., 1999), Italy (Macis and Schivardi, 2016), Portugal (Torres et al., 2013; Card et al., 2016), Brazil (Helpman et al., 2016), Sweden (Skans et al., 2008), and the US (Abowd et al., 2002; Barth et al., 2016; Song et al., 2015).

[5] For international evidence, see also Smith (2010), Blau and Kahn (2017), and Barth et al. (2017).

[6] This choice is guided by two main reasons. First, to maintain comparability with CHK who studied the years 1996-2002 and 2002-2009. My data set terminates in 2008, so I decided to shift both periods one year ahead. Second, several variables that I use in my analysis, most importantly union coverage, are only reliably recorded from 1995 onwards.

link between the rising dispersion of firm premiums and productivity.

Using a simple decomposition that allows me to evaluate the impact of firm premium differentials on gender wage inequalities in each period, I find that firm-specific pay premiums account for around 11% of the 24.7 log point gender wage gap in the 1990s, and for about 26% of the same gap in the 2000s, reflecting a 3.6 log point expansion within a decade. While sorting effects dominate firm-related gender disparities in each period (with bargaining effects being small or negative), they only represent 55% of the expansion between the 1990s and 2000s, with the remaining 45% coming from the bargaining component. Decompositions across education levels, occupations, and industries reveal substantial variation in the widening of firm gender gaps along all three dimensions, suggesting that growing firm premium inequality contributes to the slowdown of male-female wage convergence within these groups. I show that the widening of firm gender disparities is entirely attributable to differential changes in the associated firm premium distributions (rather than shifts of men and women across firms). This implies that the growth in firm gender inequality between the 1990s and 2000s represents a combined effect of i) firms with initially higher wages (where men are concentrated) featuring higher growth rates in their firm premiums compared to firms with initially lower wages (where women are concentrated); and ii) male firm premiums growing faster than female premiums in the same firms.

These findings offer an intuitive explanation for the puzzle of stagnating gender wage gaps despite continuously narrowing skill supplies: a salient growth in the dispersion of gender-specific firm premiums places workers in lower wage firms at a relative disadvantage, and this particularly affects women being overrepresented in these firms. In addition, women's returns to working for the same firms decline relative to men's, adding up to fully compensating the parallel progress of women in various dimensions of gender inequality such as skills, experience, and occupations. This basic mechanism may not be limited to the 1990s and 2000s — rather, I believe it has been concealed in earlier periods by an even faster convergence of labour market skills coupled with a slower expansion of firm premiums.

My finding of small or even negative bargaining effects counters the intuition that women negotiate (or are offered) lower wages, so I next turn to inquire into possible explanations. Focusing on the upper part of the wage distribution or on time variation in productivity within firms, I confirm that female wages are about 80-90% as responsive to productivity as male wages. These findings suggest that the premium-based bargaining effect is confounded by the presence of institutions which regulate the wage setting process especially in the lower end of the distribution. To examine this hypothesis, I narrow my focus to collective agreements as a key labour market institution in Germany. I find large and growing returns to union coverage over time, and document that the stark rise in rent-sharing noted above is generated by a closer alignment of wages and productivity inside the covered sector (in contrast to shifts from the covered to the non-covered sector). These findings highlight that the earlier view of growing wage premium inequality being related to a decline of union coverage through new firms not entering and old firms leaving coverage is by no means the full story. Rather, wage premiums expanded rapidly inside the covered sector, and I show that this expansion comes, at least partly, from a higher degree of rent-sharing coupled with a widening in firm level productivity differentials.

Turning to the implications for gender wage differentials, I find that the large and growing returns to union coverage hold up for both genders, and document that collective agreements compress the wage gap within firms. Using kernel reweighting methods, I estimate that deunionisation contributes around 27% to the rise in firm gender differentials, with a larger impact on the bottom of the distribution relative to the top. This estimate is, however, a lower bound for the overall impact of unions on growing firm gender inequality as it neglects the indirect effect of unions through their impact on the widening of firm premium heterogeneity by means of increased rent-sharing.

In a final step, I change the focus from analysing firm premium gaps over time to the impact of firm disparities over the age profile of men and women. Corroborating recent evidence in CCK and Barth et al. (2017), I show that between-firm gender differentials account for around 25-30% of the 22.6 log point expansion between the mid 20s and mid 40s. Using an event study design, I find that the rise over the lifecycle is related to childbirth, which leads to persistent wage losses of about 20 log points. Again, around 25-30% of this wage penalty comes from gradual losses in mothers' firm-specific wage component, suggesting that mothers progress slower to higher wage firms than non-mothers (and men).

2.2 Data and Descriptive Overview

2.2.1 Data Set

My analysis uses a longitudinal version of the linked employer-employee data sets, the LIAB Mover Model 9308 (Heining et al., 2012), of the German Institute for Employment Research (IAB). The mover model matches plant level survey data from the IAB Establishment Panel (EP) for years 1993 through 2008 with worker level data from the integrated employment biographies (IEB), assembled from social security notifications. The worker level data include gender, education, full time status and gross daily wages, as well as basic employer information such as location, industry, and the number of employees, aggregated from the universe of employment biographies as of June 30th each year.[7] In addition, I observe detailed survey data from the EP on a subpopulation of establishments with at least one employee subject to social security.[8] As of today, the EP is the only data source for the German labour market that allows to match firm productivity measures such as sales and cost of inputs with individual worker data, rendering this data set particularly useful for my

[7]Because wages are top-coded at the contribution limit (affecting 10-15% of male and 2-5% of female observations), I impute an upper tail of the wage distribution following closely the procedure in CHK; see Appendix A.1.3. The IEB covers all workers liable to social security contributions (about 80% of all German employees). Excluded groups are civil servants, military service, full-time students, and self-employed.

[8]I use the terms workplace, establishment, plant, and firm interchangeably, though the legal entity is an establishment.

analysis.[9]

Another distinguishing feature of the mover model lies in its sampling design which reflects the purpose of studying mobility between firms, conditional on unobserved worker and firm heterogeneity (see Appendix A.1 for details). The starting point of the sample construction are all workers who can be linked to an establishment from the EP. From this group, all 'movers' are selected, defined as all workers employed in their main job as of June 30th in two different years and EP establishments. For each workplace in the associated set of connected establishments, up to 500 further employment biographies with a link to that workplace are randomly sampled and added to the data (if an establishment employs fewer workers, all are sampled). The final sample then consists of all movers plus all workers who either do not move or move to an establishment not participating in the EP. It covers roughly three quarters of person-years, but less then 5% of all establishments in the data.

The majority of establishments and some 25% of person-years enter the sample because I observe complete employment biographies with identifiers for all employers a matched person worked for in the 1993-2008 period. For these establishments, I do not have survey information but administrative information generated from the IEB, and I also observe a much smaller (random) share of their workforce.[10] I refer to the full employment biographies including both types of establishments as the overall worker sample, and, except for sections 2.2.3 and 2.4.6, divide this data set into two overlapping intervals of comparable length: 1995-2001 (1990s) and 2001-2008 (2000s). Table A.2 summarises these samples (by gender and period) together with subsamples associated with EP establishments. The total sample size varies between 10.6 and 12.0 million male and about 4.7 to 5.1 million female worker-years, associated with some 1.9 million men and 1.0 million women in each period.

Although the sample design facilitates a dense mobility network — e.g., about 50% of EP establishments employ more than 25 female movers and about 60% more than 25 male movers in each period — the fact that I do not observe the universe (as in CHK) might raise concerns of excess sampling error and lack of representativeness. I attempt to address both concerns through comparisons with other data sources, such as CHK for the trends in wage dispersion and census data for the trends in productivity dispersion. Overall, these comparisons are encouraging.[11]

[9]One exception is Bender et al. (2016) who use a unique match between some 360 German manufacturing plants in the World Management Survey and the IEB. The financial information for these plants come from the ORBIS data base, which is clearly superior to survey data. Unfortunately, a broader match between ORBIS and the IEB does not exist.

[10]Figure A.1 shows how the ratio of observed to actual full-time workers varies in firm size for different analysis samples explained below. To measure the actual number of full-time workers, I use the number of full-time workers provided from the IEB averaged over the past and current year.

[11]Ideally, I would compare my estimates directly with the CHK effects. Unfortunately, although the IAB has taken steps to make the original CHK effects available for several LIAB data sets, the mover model is not among them.

Table 2.1: Summary Statistics of Largest Connected Sets and Dual-Connected Sets

	1995-2001				2001-2008			
	Largest connected set (for AKM)		Dual-connected set		Largest connected set (for AKM)		Dual-connected set	
	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Observations								
Person-years	10,391,535	4,406,789	8,952,504	4,096,533	11,758,569	4,839,439	10,169,739	4,507,696
Percent of overall sample	*98.2*	*94.6*	*84.6*	*88.0*	*98.1*	*94.1*	*84.9*	*87.6*
Workers	1,805,964	898,950	1,657,628	869,301	1,832,141	893,349	1,682,394	862,494
Percent of overall sample	*97.2*	*92.4*	*89.2*	*89.3*	*97.2*	*91.9*	*89.3*	*88.7*
Establishments	330,720	182,061	88,968	88,968	325,651	182,528	93,306	93,306
Percent of overall sample	*85.0*	*70.8*	*22.9*	*34.6*	*85.1*	*70.4*	*24.4*	*36.0*
Number of job-spells per worker	1.60	1.49	1.39	1.37	1.62	1.51	1.41	1.39
Worker characteristics (low educated omitted)								
Mean age	39.2	37.5	39.7	37.8	41.5	39.9	41.8	40.2
Mean tenure (years)	8.5	7.3	9.1	7.6	9.5	8.2	10.1	8.5
Share medium education (apprenticeship)	0.67	0.63	0.65	0.62	0.65	0.60	0.64	0.59
Share high education (college/university)	0.19	0.19	0.20	0.19	0.23	0.25	0.24	0.25
Wages								
Mean of log daily wage	4.553	4.318	4.586	4.339	4.579	4.344	4.613	4.366
Std. dev. of log daily wage	(0.369)	(0.381)	(0.358)	(0.367)	(0.416)	(0.435)	(0.403)	(0.421)
100 x Log gender wage gap	23.5		24.7		23.5		24.7	
Percent of gender gap in overall sample	*94.8*		*99.6*		*94.0*		*98.8*	
Workplace characteristics								
Mean firm size (full-time)	1,429	768	1,645	824	1,368	693	1,569	742
Share of female coworkers	0.21	0.52	0.24	0.48	0.21	0.50	0.23	0.47
Share at all-male firms	0.09	–	–	–	0.08	–	–	–
Share at all-female firms	–	0.06	–	–	–	0.05	–	–

Note: Table shows person-year weighted summary statistics. Entries in the first two columns in each period refer to the largest connected set for each gender. The next two columns describe the dual-connected set (see main text for definitions of connected sets).
Source: LIAB Mover Model 9308

2.2.2 Analysis Samples

My empirical analysis is based on the main job (unique person-year observation) of West German men and women in full-time employment, aged 20 to 60, not in training nor marginally employed, and earning a daily wage above 10 Euros.[12] From the overall worker sample outlined above, I define two subsamples for the analysis, summarised in Table 2.1. The first two columns show the samples used in the AKM estimations. I limit attention to the largest connected set (Abowd et al., 2002), which covers around 98% of male and 94-95% of female person-years in the overall worker sample. These shares are high enough to discuss the main sample characteristics based on the connected sets.

The fraction of women among all full-time workers is around 30% in each period, reflecting a persistent gender gap in full-time participation rates (when including part-time workers, this ratio is near 50%). Women are 1.5-2.0 years younger than men, less tenured by about 1.0-1.5 years, and less educated than men in the 1990s, but appear to close this gap in the 2000s. Despite this convergence, the wage gap between men and women is 23.5 log points and virtually constant over time (24.7 log points in the overall worker sample). Parallel to this stagnation, the standard deviation of wages rose sharply by around 4.6 log points for men, and 5.4 log points for women. Moving to workplace characteristics, women work at smaller firms that feature a larger share of female coworkers (52% vs. 21%).[13] Another indicator of workplace segregation is the share of women (men) employed at all-female (all-male) firms. I find this fraction to be relatively low: less than 10% of men and women in connected sets work at firms that employ no worker from the other gender over time.[14]

As noted in CCK, single-gender firms violate the common support assumption in decompositions outlined below, i.e., there exists no counterfactual male wage at all female firms, and vice versa. Most of my analysis will therefore be based on samples summarised in the next two columns, which impose that firms belong to the connected sets of each gender (dual-connected). Around 85-90% of all person-year observations, and around 25-35% of all establishments in the data enter this sample. While the baseline characteristics are similar, this restriction leads to a somewhat larger gender wage gap, more equal to the gender gap in the overall worker sample, and exceeding the wage gap in the largest connected sets. This suggests that pay differentials between male and female single-gender firms are relatively small.

[12]See Appendix A.1.2 for a definition of the main job. The restriction to full-time employment is necessary to reduce an hours bias in daily wages since the data lack information on working hours (see Appendix A.6).

[13]The first observation has been made in several studies for Germany (Gürtzgen, 2009, 2012; Antonczyk et al., 2010), and it differs from studies for the US, UK, and for Portugal, where women tend to work in larger firms (Papps, 2012; Mumford and Smith, 2009; CCK). Regarding the second observation, CCK report a female share for the male (female) sample of 24% (70%) for the period 2002-2009.

[14]A comparable statistic for Portugal reaches substantially larger values of 20% based on the universe of workers.

2.2.3 Overview of Trends in Gender Gaps, Firm Premiums, and Productivity

Based on CCK's model of gender disparities driven by productivity differentials across firms, I analyse below how a widening in the dispersion of firm productivity contributes to the recent stagnation of gender gaps through its impact on male and female firm premiums. To set the stage, this section summarises the major developments in these key statistics over the past two decades.

The Pervasive Stagnation of Gender Inequality: Figure 2.1 explores a series of gender gaps for the West German labour market since 1985. I present long run trends for unconditional gender gaps together with estimates of a variety of conditional gender gaps. The basic model includes human capital controls, and is sequentially augmented by fixed effects for occupations (~330), industries (~254), and establishments. All series are normalised to 1995 as a focal point of my analysis.[15] The figure illustrates a rapid convergence of gender inequality during the 1980s and early 1990s, culminating in a sudden trend break and a rather flat development from the mid 1990s onwards. Note that the indexation conceals level differences across the various gender gaps. As of 1995, the different measures span from about 25 log points for the raw gender gap to 16.5 log points for men and women in the same firms and with similar human capital. Comparing the post-1995 trends across the different series points to another interesting conclusion: while the role of human capital is steadily declining, within firm wage inequality follows a flat trend throughout 2008. One interpretation of this pattern is that growing workplace segregation contributes to the persistence of gender inequality in recent years.

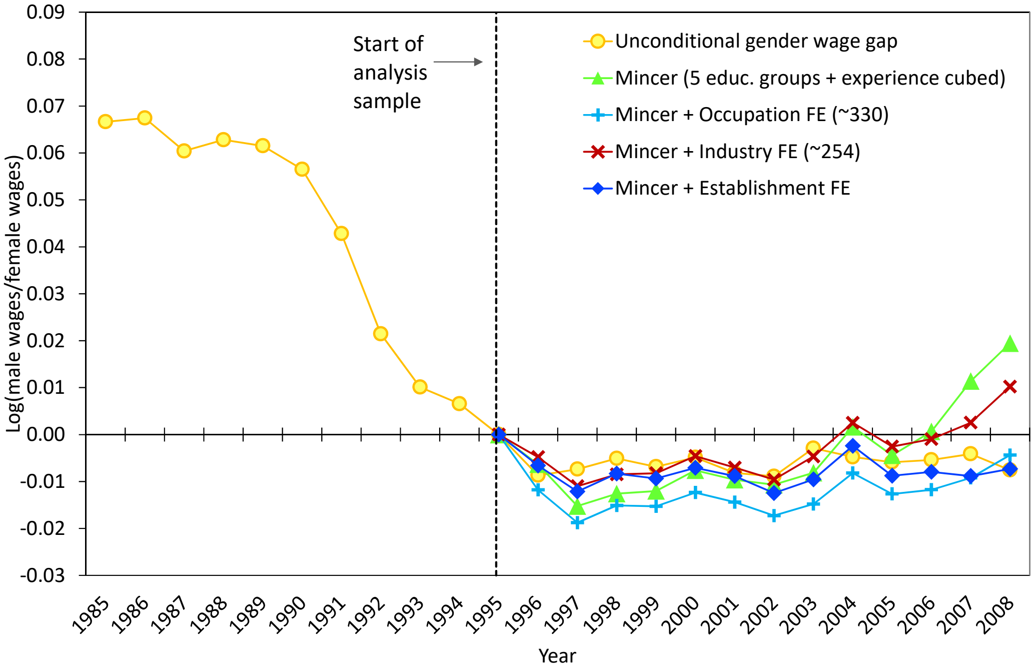
The Expansion of Firm Premiums and Productivity: The growth in firm-specific wage heterogeneity represents a key source of rising wage inequality over the past two decades. Two main developments contribute to this trend: a growing wage dispersion between firms, and an increased sorting of higher quality workers into higher wage firms (CHK). Jointly, these trends account for roughly two thirds of the total wage expansion between 1995 and 2008.[16] A natural question is *why* workplace-specific wage heterogeneity expanded in recent decades: Was it generated by growing productivity differentials, or by changes in rent-sharing, or a combination of both? I will show that both factors are at play, but for now I focus on the broader, macro level correlations. To motivate, note that the variance of mean log value added per worker in the EP rose from 0.35 in 1995 to 0.40 in 2008, with a 90/10 percentile spread rising from 1.38 to 1.50.[17] For a more detailed summary, 2.2

[15] This analysis is based on the overall worker sample described in Table A.2, and long run trends for the pre-1995 period are obtained from the sample of integrated employment biographies (SIAB 7510), a 2% random sample of workers subject to social security. The human capital index includes five education dummies and a cubic polynomial in experience.

[16] This figure is obtained from a basic variance decomposition of models that interact the human capital index from above with a gender dummy, and include firm effects. As shown below, similar conclusions follow from more sophisticated models that take unobserved worker heterogeneity explicitly into account.

[17] Throughout this paper, I measure value added per worker as the difference between sales and cost of inputs,

Figure 2.1: Trends in Gender Wage Gaps Over Time



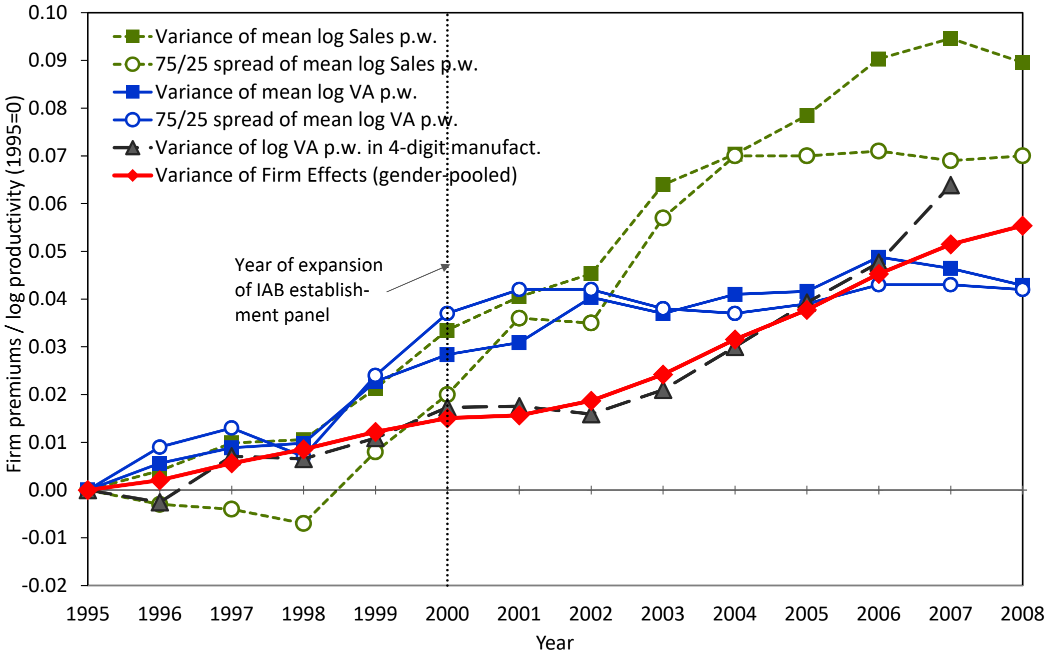
Note: Figure shows unconditional and conditional gender wage gaps. Each line corresponds to a coefficient on a dummy for gender (male=1) in a linear wage regression estimated on annual cross-sections. The series from 1980 onwards is based on the Sample of Integrated Employment Biographies 7510 (SIAB). The series starting 1995 are obtained from the LIAB Mover Model 9308. Mincer covariates include dummies for 5 education categories and a cubic polynomial in experience. Additional fixed effects are included as indicated in the figure.
Source: LIAB Mover Model 9308; SIAB 7510

plots the variances and 75/25 percentile gaps of mean log value added per worker and mean log sales per worker together with the dispersion of firm-specific wage premiums over time (normalised to 1995). A striking conclusion from this figure is a very high and pervasive correlation between productivity and wage dispersion. This finding lines up well with recent evidence for the US (Barth et al., 2016), and it strongly suggests a possible link between these two developments.

As I deal with survey data, a natural concern is that these developments reflect changes in the sampling frame rather than actual trends in productivity dispersion. Most relevant to my analysis, the EP nearly doubled the number of surveyed establishments in 2000, with a particular focus on small firms. These firms are presumably less productive than the average firm in the data (Foster et al., 2008), suggesting a sudden widening of lower tail inequality. Reassuringly, the various measures of dispersion depicted in Figure 2.2 indicate relatively smooth trends over time. Similar conclusions emerge from tracking different percentiles over time (Figure A.4), or focusing on residual dispersion that is cleaned from variation across industries, firm sizes, and federal states. To further substantiate these trends, I obtained a census-based estimate of the variance of log value added per worker for four-digit manufacturing industries (see Figure 2.2). Although there are several limitations

divided by the number of full-time employees; for further details, see section A.2.1 of the Appendix.

Figure 2.2: Trends in Firm Premium Heterogeneity and the Dispersion of Average Productivity per Worker Between 1995 and 2008



Note: Figure shows measures of dispersion for productivity and establishment wage premiums. The variance of establishment premiums (red diamonds) is estimated from annual regressions that include a cubic polynomial of experience and dummies for 5 education groups, fully interacted with a gender dummy. Productivity measures are calculated for establishments with at least one year of value added/sales (EP sample), and are averaged across all years a firm is observed. Value added and sales are measured in thousands of real year 2000 Euros, using an aggregate World Bank GDP-deflator. The variance of log value added per worker in 4-digit manufacturing industries is computed from value added data of the German Federal Statistical Office for the period 1995-2007. All lines are weighted by employment and scaled to 1995=0.
Source: LIAB Mover Model 9308; Federal Statistical Office 1995-2007

to this measure, it is encouraging that it exhibits a very similar trend increase over time.[18]

2.3 Modelling Framework

2.3.1 Econometric Approach

The previous patterns suggest a link between trends in gender wage differentials and firm-specific wage premiums that is sourced in productivity differentials. To study this relationship more formally, I draw on a simple wage bargaining model, first developed by CCK, that gives rise to an AKM wage decomposition.[19] In particular, denoting the gender of worker i by $G(i) \in \{M,F\}$, and the identity of his or her employer in a given year $t \in \{1 \dots T\}$ in

[18] Tables are published by the German Federal Statistical Office and available online. Consistent industry codes are available for 1995-2007, so I pick 2007 as the last year. Comparable data for *all* industries is only available for the 2-digit level. In this case, the variance rises by 4.4 log points, again very close to the 4.3 log point rise in the EP (Figure 2.2).

[19] See section A.3 of the Appendix and CCK for a possible microfoundation.

period p by $J(i,t)$, I implement models of the form

$$w_{it} = \alpha_{i,p} + \psi^{G(i)}_{J(i,t),p} + X'_{it}\beta^{G(i)}_{p} + r_{it} \tag{2.1}$$

where $\alpha_{i,p}$ is a worker effect measuring i's (fully portable) earnings capacity, $X'_{it}\beta^{G(i)}_{p}$ is a covariate index capturing gender-specific returns to time-varying worker level observables, $\psi^{G(i)}_{J(i,t),p}$ is a gender-specific firm effect paid to all workers employed at firm j, and r_{it} is a composite error term (Appendix A.3). I estimate the AKM models via OLS separately for each gender and period, thus allowing for unrestricted firm effects, person effects, and returns to covariates across genders *and* over time.

I use gender- and period-specific firm premiums as the basis for my analysis, though I also investigate two alternative sets of AKM models as robustness: one that identifies gender-pooled (but period-specific) firm effects, and one that identifies period-pooled (but gender-specific) firm effects. In the first case, I restrict firm gender disparities to segregation across firms (sorting) and eliminate within-firm gender disparities (bargaining). This is a useful counterfactual to assess by how much the contribution of the firm to changes in the gender wage gap would be understated if the bargaining channel was omitted. In the second case, I restrict firm premiums to be constant over time, thus eliminating a key driver of the recent expansion in aggregate wage inequality: rising workplace heterogeneity. This yields a straightforward counterfactual to evaluate the extent to which changes in firm gender inequality are driven by an expansion of the firm premium distribution (as opposed to a relocation across firms).

For OLS to yield unbiased estimates of the worker and firm effects, the assignment of workers to firms must be uncorrelated with the error term (exogenous mobility assumption). CHK develop several exercises to probe the validity of this assumption, and conclude that it seems to be (approximately) satisfied. As my analysis uses a more restrictive data set than CHK, I verify the proximate validity of the assumption by replicating their exercises in Appendix A.4. The results are remarkably similar.

2.3.2 The Role of Firm-Specific Wages for Gender Wage Inequality

Using the estimated firm effects and an appropriate normalisation to make them comparable across genders and over time, I evaluate the role of gender-specific firm premiums for male-female wage gaps based on the following period-specific decompositions (I omit the $J(i,t)$-subscript for clarity)

$$E\left[\hat{\psi}^M_p|M_p\right] - E\left[\hat{\psi}^F_p|F_p\right] = E\left[\hat{\psi}^M_p - \hat{\psi}^F_p|M_p\right] + E\left[\hat{\psi}^F_p|M_p\right] - E\left[\hat{\psi}^F_p|F_p\right] \tag{2.2}$$

$$= E\left[\hat{\psi}^M_p - \hat{\psi}^F_p|F_p\right] + E\left[\hat{\psi}^M_p|M_p\right] - E\left[\hat{\psi}^M_p|F_p\right] \tag{2.3}$$

The first component in each line measures the average gender gap in firm surplus, weighted by either the distribution of male (2.2) or female (2.3) employment. It measures by how much the gender gap would change if women received the same firm premium as men, and can be interpreted as a bargaining effect. The second component measures by how much the gender wage gap would change if women worked in the same firms as men, weighted by either female (2.2) or male (2.3) firm effects, and it is interpreted as a sorting effect. My basic empirical approach is to perform these decompositions separately for each period, and to analyse how each component changes over time. Building on the key findings, I then turn to investigate several explanations for the main trends established in this exercise.

As explained below, I use an estimate of the average (gender-specific) firm premium paid by low productive firms — measured by mean log value added — to obtain a normalisation that justifies an interpretation in terms of firm rents. The decomposition shows that the choice of an appropriate reference group affects the overall contribution of firm premiums to the gender wage gap as well as its changes over time by altering the magnitude of the bargaining channel (Oaxaca and Ransom, 1999). This is important for two reasons. First, establishments from the EP tend to have more positive firm effects than the average establishment in the data, which might downward bias the bargaining channel if gender differences expand in the level of (potential) surplus. Second, as the EP expanded coverage in 2000, changes in the sampling frame might generate spurious adjustments in the bargaining channel. To address these concerns, I verify my results under several alternative normalisation schemes that exploit different sources of information in the data, some of them not relying on establishments from the EP.

For conciseness, the discussion in the main part of this paper focuses on the decomposition described by eq. (2.2), which assigns women the job distribution of men. I presume that this is a more relevant counterfactual because female labour supply can be affected more easily by policy interventions (e.g., through changes in the provision of public childcare and job protection schemes), and because policy makers might be particularly concerned with the relative position of women in male dominated firms. The main results for the reverse counterfactual are similar and summarised in Table A.8 of the Appendix.

2.4 Results

2.4.1 Estimation of Worker-Firm Models

As a first step of my analysis, I estimate models of eq. (2.1) for each gender and period, allowing for unrestricted worker effects, firm effects, and returns to observables.[20] Having

[20] The covariate index contains five education dummies, fully interacted with year dummies and a cubic polynomial in age (CHK). I omit the first year effect in each education group, and the linear term in age, with the age profile normalised to be flat at age 40. The latter ensures that I obtain similarly scaled values of the worker effects, α_i, and the covariate index, $X_{it}'\beta^g$, as CHK. Card et al. (2016) provide a comprehensive discussion of identification issues arising in the estimation of AKM models. I perform estimations using Stata's *a2reg* command that sets the last firm effect to zero (Ouazad, 2007).

Table 2.2: Summary of Estimation Results for AKM Models

	1995-2001				2001-2008			
	Male		Female		Male		Female	
	(1)		(2)		(3)		(4)	
Estimation results								
# of person-effects	1,805,964		898,950		1,832,141		893,349	
# of firm-effects	330,720		182,061		325,651		182,528	
Standard dev. of person-effects (α)	0.295		0.304		0.320		0.339	
Standard dev. of firm-effects (ψ)	0.156		0.194		0.196		0.231	
Standard dev. of covariates ($X\beta$)	0.080		0.060		0.065		0.049	
Correlation of person- and firm-effects	0.041		-0.046		0.094		-0.019	
… <5 movers	-0.187		-0.280		-0.113		-0.268	
… ≥ 5 movers	0.066		0.038		0.119		0.073	
… ≥ 25 movers	0.064		0.088		0.118		0.126	
Correlation of M and F firm-effects	0.571				0.665			
Model fit								
Adjusted R2 of AKM model	0.914		0.885		0.926		0.892	
Root MSE of AKM model	0.108		0.129		0.113		0.143	
Fit of match effects model								
Adjusted R2 of match-effects model	0.932		0.903		0.941		0.908	
Root MSE of match-effects model	0.096		0.118		0.101		0.132	
Standard dev. of match-effects	0.051		0.053		0.054		0.057	
in % of residual variance of AKM	*(28.4)*		*(22.1)*		*(27.6)*		*(20.7)*	
Variance decomposition								
Var(w)	0.136	*100*	0.145	*100*	0.173	*100*	0.189	*100*
Var(α)	0.087	*64*	0.092	*64*	0.102	*59*	0.115	*61*
Var(ψ)	0.024	*18*	0.038	*26*	0.038	*22*	0.053	*28*
Var($X\beta$)	0.006	*5*	0.004	*2*	0.004	*2*	0.002	*1*
$2 \times$ Cov(α, ψ)	0.004	*3*	-0.005	*-4*	0.012	*7*	-0.003	*-2*
$2 \times$ Cov(α, $X\beta$)	0.003	*2*	0.003	*2*	0.003	*2*	0.004	*2*
$2 \times$ Cov(ψ, $X\beta$)	0.002	*1*	0.001	*1*	0.003	*1*	0.002	*1*
Var(r)	0.009	*7*	0.013	*9*	0.011	*6*	0.016	*8*

Note: Table shows results from OLS estimations of AKM models specified in eq. (2.1). All models are estimated on the largest connected sets as described in the main text. Columns 1-2 show the results for the first interval, and columns 3-4 for the second interval. In each interval, AKM models are estimated separately for each gender. The correlations of male and female firm effects and between worker and firm effects for different numbers of movers were calculated for the subset of dual-connected firms. The match effects model contains a dummy for each job-match.
Source: LIAB Mover Model 9308

established that my (sample-based) results replicate the main inequality trends documented in CHK, I then use the estimated firm effects for workers and firms in the dual-connected sets to study the role of gender inequalities in firm rents for the overall gender wage gap in each period and over time. Table 2.2 summarises the main estimation results for each of the four models estimated on the largest connected sets. The first set of rows reports standard deviations of worker effects, firm effects, and the covariate index as well as the correlation between worker and firm effects. The results are largely in line with evidence reported in CHK. While worker effects represent the largest component of wage inequality in each period, followed by firm effects, the latter account for the largest *increase* over time. The correlation of worker and firm effects, a measure of assortative matching, reveals a strong rise among both genders over time (e.g., by 130% among men). These correlations are presumably downward biased by limited mobility as for a large number of establishments I only observe few movers (section 2.2.1). In line with this, the next rows document considerably higher correlations for subsamples with more movers per establishment. However, this result should be interpreted with caution as it might confound the effect of changing the number of movers with true changes in the correlation, e.g., if larger establishments employ more movers *and* feature (truly) stronger positive matching.[21]

In the middle panel, I summarise model fit statistics and specification checks, documenting an equally good model fit as in CHK. Further model diagnostics along these lines, all leading to similar conclusions, are discussed in section A.5 of the Appendix.

To demonstrate the importance of growing firm wage heterogeneity, the bottom rows decompose the overall variance of log wages into the components implied by eq. (2.1). A summary measure subsuming all terms related to workplace heterogeneity is given by $Cov(w, \psi^g)$ from the associated decomposition: $Var(w) = Cov(w, \alpha) + Cov(w, \psi^g) + Cov(w, X\beta^g) + Cov(w, r)$. Evaluating this expression in terms of cross-period changes, $\Delta Cov(w, \psi^g) / \Delta Var(w)$, reveals that around 62% (43%) of the rise in male (female) wage inequality is attributable to workplace related wage components — affirmative of firm heterogeneity being a key driver of recent changes in the wage structure.

2.4.2 Normalisation

The firm effects cannot be compared directly across estimation samples, as they are only identified up to a constant within a connected set of firms (Abowd et al., 2002). Building on the assumption that rents at low surplus firms are on average zero, I next use a mapping between the estimated firm effects and a measure of firm surplus, mean log value added, to identify a threshold value below which firms pay zero average rents (for details, see CCK). I implement this procedure for a subsample of EP establishments with at least one year of value added (Table A.2). These establishments employ one-quarter to one-third of all workers, and are associated with a higher but similarly stable gender wage gap. Interestingly, the average male employer is about 4 to 6 log points more productive than the average female employer. Figure 2.3 illustrates the relationship between mean log value added and firm-specific wage premiums for men and women, assigning firms into 100 bins of value added and calculating averages within each bin.[22] The cutoffs are located at 3.2 in the 1990s and 3.1 in the 2000s, corresponding to an annual value added per worker of 20-25 thousand Euros — roughly equal to the mean wage bill in a typical German low wage sector, the hospitality industry. Using the mean gender-specific firm premiums of firms below these thresholds in the EP data

[21] A cleaner assessment would be to change the number of movers while holding the structure of establishments constant. This is infeasible with the data at hand, but Andrews et al. (2012) conduct precisely that experiment, showing a similar concave shape of the measured correlation in the number of movers per firm as in Table 2.2. To further check the limited mobility hypothesis, I estimate period-pooled AKM models (Table A.5). The correlations rise to 0.123 in the male sample, and 0.043 in the female sample, in line with a downward bias in gender- and period-specific estimates.

[22] CCK document a similar nonlinear pattern Portugal. For this exercise and the analysis in sections 2.4.4 and 2.4.5, I focus on the distribution of mean log value added per worker below 5 since, empirically, the association between firm premiums and productivity is zero above that level.

set, I find that roughly 16-22% of workers in the dual-connected set fall into the zero-surplus reference group. Additional descriptives on low and high productivity firms are provided in Table A.3. As expected, firms with low value added are on average smaller, employ more women, are overrepresented in service industries, and have lower average revenues per worker.

2.4.3 Baseline Results

Basic Decompositions: Using the normalised firm premiums, I next turn to decompose the gender gap in firm premiums. Table 2.3 summarises the main results for the 1990s (col. 1-4), the 2000s (col. 5-8), and the differences across periods (col. 9-12). The first two rows display the unconditional wage levels of men and women, and the normalised firm premiums. The average firm rent of men is around 10.3% in the 1990s and 17.6% in the 2000s, while the corresponding values for women are 7.5% and 11.2%. Columns 4 and 8 show the implied firm gender gap, suggesting that between the 1990s and 2000s, firm-specific gender disparities rose from 0.028 (11.3% of the overall gender gap) to 0.064 (25.9%). In view of a stable 24.7 log point wage gap in both periods (row 1), this finding suggests that without an expansion of firm-specific gender pay differentials (ceteris paribus), the wage gap between men and women would have declined by around 3.6 log points, or about 15% over time.[23]

Rows 3 and 4 show how the firm-related gender gap can be decomposed into a sorting and a bargaining component. These entries are based on eq. (2.2), i.e., they use female firm rents to evaluate the sorting component, and male employment to evaluate the bargaining component. The results suggest that the primary source of firm rent differentials between genders is an underrepresentation of women at higher wage firms: in the 1990s, the sorting component amounts to 0.042, which is partially compensated by a negative bargaining effect of -0.014. The dominant sorting effect persists in the 2000s, where it increases to 0.063 coupled with a roughly zero (0.001) bargaining effect. Together, these findings suggest that while sorting effects govern the firm gender gap in each period, both components contribute to the net increase of the firm gender gap over time, with sorting effects accounting for 55% (0.02/0.036) and bargaining effects for some 45% (0.015/0.036).[24]

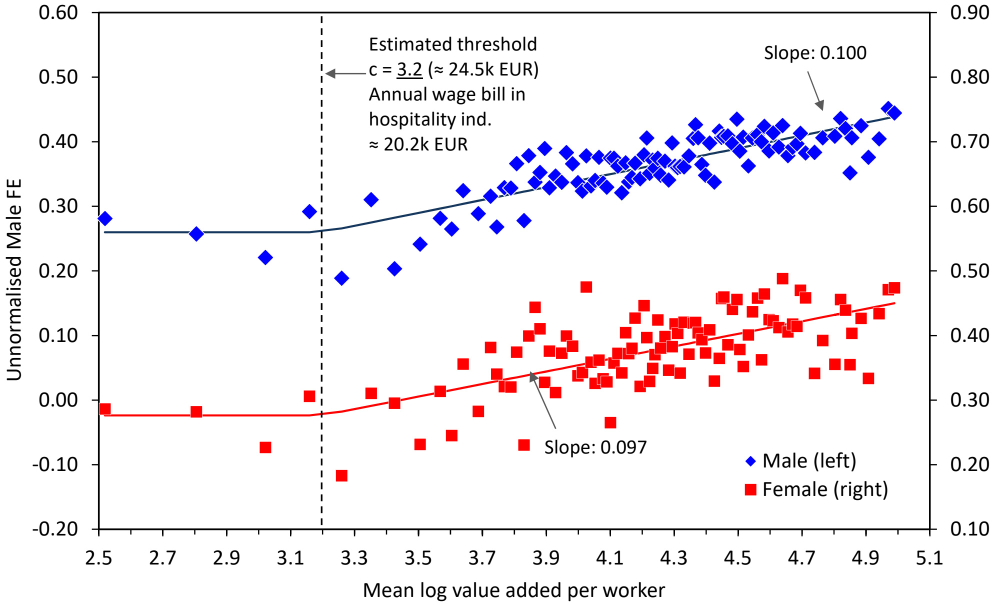
The widening of male-female firm premium inequality as a principal source of persistent gender wage gaps over the past two decades represents a key finding in this paper. In the following analysis, I show that this expansion has occurred against a stable distribution

[23]From eq. (2.1) it follows that changes in $X'\beta^g$ and/or α compressed the gender wage gap. In section A.7 of the Appendix, I apply Gelbach's decomposition to gender-pooled AKM models to gauge the contribution of each wage component of eq. (2.1). These models restrict the impact of firms to the sorting effect, which increases by about 1.8 log points over time, and they show that one third of this is compensated by a convergence in $X'\beta^g$ and two thirds by α.

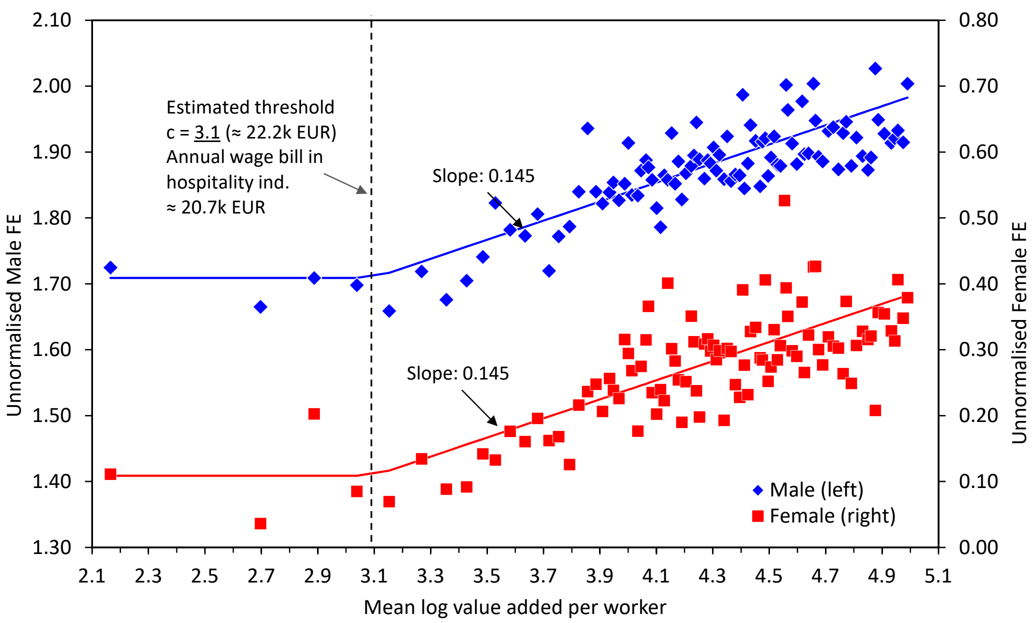
[24] Panel B presents decompositions for different levels of education. Firm premiums are rising in education, reflecting positive assortative matching between skills and productivity. Comparisons over time reveal a pervasive growth in firm premium inequality between genders across all skill groups. Surprisingly, the decompositions do not assign a systematically higher bargaining component to university graduates than to lesser educated workers.

Figure 2.3: Male and Female Firm Effects Grouped into 100 Bins of Mean Log Value Added per Worker

A. 1995-2001



B. 2001-2008



Note: Figure shows unnormalised firm effects of men (left scale) and women (right scale) grouped into 100 bins of mean log value added. Sample is based on firms with 1+ year of log value added that belong to the dual-connected set, and mean log value added per worker is trimmed above 5. Bins are weighted by person-years. Estimated firm effects were obtained from AKM-regression reported in Table 2.2. Estimation of the kinkpoint is based on the procedure described in the main text and CCK.
Source: LIAB Mover Model 9308

Table 2.3: Decomposition of Changes in the Gender Gap of Firm Premiums Across Periods

	1995-2001				2001-2008				Differences			
	Male	Female	Δ Male-Female		Male	Female	Δ Male-Female		Male	Female	Δ Male-Female	
			Change	Share			Change	Share			Change	Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. All workers												
Mean log real daily wages (unadjusted)			0.247	100.0			0.247	100.0			0.000	0.0
Firm-specific wage premium	0.103	0.075	0.028	11.3	0.176	0.112	0.064	25.9	0.073	0.037	0.036	14.6
Sorting (using female FE's)	0.118	0.075	0.042	17.2	0.175	0.112	0.063	25.4	0.057	0.037	0.020	8.3
Bargaining (using male employment)	0.103	0.117	-0.014	-5.8	0.176	0.175	0.001	0.3	0.073	0.058	0.015	6.1
Panel B. Skill Groups												
i. Missing / Primary												
Mean log real daily wages (unadjusted)			0.223	100.0			0.226	100.0			0.003	1.3
Firm-specific wage premium	0.093	0.068	0.024	10.9	0.151	0.095	0.056	24.9	0.058	0.027	0.032	12.9
Sorting (using female FE's)	0.107	0.068	0.038	17.2	0.150	0.095	0.055	24.3	0.043	0.027	0.017	6.7
Bargaining (using male employment)	0.093	0.107	-0.014	-6.3	0.151	0.150	0.001	0.6	0.058	0.043	0.015	6.2
ii. Apprenticeship												
Mean log real daily wages (unadjusted)			0.204	100.0			0.201	100.0			-0.002	-1.2
Firm-specific wage premium	0.103	0.074	0.028	13.9	0.173	0.109	0.063	31.5	0.070	0.035	0.035	14.2
Sorting (using female FE's)	0.115	0.074	0.040	19.8	0.169	0.109	0.059	29.6	0.054	0.035	0.019	7.7
Bargaining (using male employment)	0.103	0.115	-0.012	-5.9	0.173	0.169	0.004	1.9	0.070	0.054	0.016	6.5
iii. College / University												
Mean log real daily wages (unadjusted)			0.276	100.0			0.292	100.0			0.016	5.7
Firm-specific wage premium	0.117	0.096	0.021	7.6	0.195	0.136	0.059	20.3	0.079	0.040	0.038	15.5
Sorting (using female FE's)	0.141	0.096	0.046	16.6	0.204	0.136	0.068	23.4	0.063	0.040	0.022	9.1
Bargaining (using male employment)	0.117	0.141	-0.025	-9.0	0.195	0.204	-0.009	-3.1	0.079	0.063	0.016	6.5

Note: Table shows the decomposition of the gender gap in firm premiums into sorting and bargaining effects according to eq. (2.2). Sample contains all firms in dual-connected sets in each sample period. Columns 1-4 show results for the 1990s, columns 5-8 for the 2000s, and columns 9-12 calculate changes across periods. In each set of columns, the first two columns calculate the (counterfactual) levels of firm rents using eq. (2.2). The next column calculates the difference, and the last column reports percent shares of row 1 in each panel. Entries in column 12 refer to the percent change relative to the level in the 1990s. For example, 3.6/24.7 = 14.6%.
Source: LIAB Mover Model 9308

of men and women across firms, and establish that the key driver of growing firm gender differentials are changes in the distributions of gender-specific firm premiums that magnify extant inequalities between men and women.

Alternative Normalisations: A key issue for the decompositions is the choice of an appropriate reference group to compare wage premiums between genders and over time. To probe the sensitivity of my results, Table A.9 reproduces the baseline decompositions of Table 2.3, panel A, under several alternative normalisations that exploit different sources of information in the data. In panel B, I use the distribution of sales per worker, assuming that firms in the bottom of the sales distribution pay zero rents on average.[25] Sales are available for a larger number of establishments, and I can thus draw on a larger sample for the normalisation. The next two panels use the mean firm premium in industries for which the assumption of zero average rents is *a priori* satisfied: the hospitality industry and business services in food, cleaning, security, and logistics. As industry codes are available for all establishments in the data, these normalisations are not restricted to EP establishments. The average wage bill in the hospitality industry is around 10-15% lower than the estimated thresholds derived in Figure 2.3, and the average firm premium is lower than in all other industries (Figure A.13).

Comparisons across the last columns draw a consistent picture of widening firm gender differentials over time, with my preferred normalisation in panel A suggesting a midway between normalisations based on sales and business service providers.

Sources of Rising Firm Inequality Between Genders: The decompositions reveal a substantial rise of the gender gap in firm premiums and its contribution to the overall wage gap since the 1990s. To interpret these changes, it is useful to understand to what extent they are generated by changes in the gender-specific firm premium distributions (a wage structure effect), or by shifts in the relative employment shares (a relocation effect). For example, the same rise in the sorting component could arise from higher wage firms (where men are concentrated) featuring higher wage growth, indicating that changes in firm-specific pay policies are important, or by men being relatively more likely than women to move to higher wage firms, indicating that changes in the underlying matching process are important.

To investigate this, I construct counterfactual distributions of gender-specific firm premiums by reweighting men and women to their sorting and bargaining distributions of the 1990s (DiNardo et al., 1996 [DFL]), using as counterfactual weights the female employment shares within deciles of firm premiums and bargaining effects. The results are summarised in Table 2.4. Panel A replicates the observed distribution of wages and firm rents for the mean (Table 2.3) and for selected percentiles across the distribution, and panel B shows the distributions obtained after assigning men and women in the 2000s their corresponding positions in the 1990s.[26] If changes in the returns to working for high/low wage firms explained

[25] I use the bottom 6% of the sales distribution to define the normalisation. This threshold is *ad hoc*, but alternative values below 10% lead to similar results.

[26] Small deviations between the results in Table 2.4 and Table 2.3 arise because in order to perform the DFL decomposition, I assign the year 2001, which is contained in both sample intervals, to the 2001-2008 period.

Table 2.4: DFL-Decomposition of Changes in Gender Wage Gaps and Firm Premium Differentials Between Genders Over Time

	1995-2001			2001-2008			Period change in Δ M-F	Percent of change in GWG (7) explained by	
	Male	Female	Δ M-F	Male	Female	Δ M-F		Distribution	Returns
	(1)	(2)	(3)	(4)	(5)	(6)	(7)=(6)-(3)	(8)	(9)
Panel A. Observed Distribution									
Mean log real daily wage	4.582	4.335	0.248	4.613	4.366	0.247	-0.001		
Mean firm-specific wage premium	0.104	0.075	0.028	0.176	0.112	0.064	0.035		
Percentiles of firm-specific wage premium									
25th	0.030	0.009	0.021	0.089	0.023	0.066	0.045		
50th	0.125	0.089	0.035	0.209	0.128	0.081	0.045		
75th	0.191	0.170	0.021	0.297	0.237	0.060	0.039		
Panel B. Counterfactual Distributions - reweighted to 1995-2001 sorting and bargaining									
i. Using male firm wage premiums									
Mean log real daily wage	4.582	4.335	0.248	4.636	4.387	0.249	0.001		
Mean firm-specific wage premium	0.104	0.075	0.028	0.189	0.125	0.064	0.035	-0.1%	100.1%
Percentiles of firm-specific wage premium									
25th	0.030	0.009	0.021	0.101	0.041	0.060	0.039	12.7%	87.3%
50th	0.125	0.089	0.035	0.217	0.134	0.083	0.048	-6.0%	106.0%
75th	0.191	0.170	0.021	0.298	0.240	0.058	0.037	5.8%	94.2%
ii. Using female firm wage premiums									
Mean log real daily wage	4.582	4.335	0.248	4.636	4.387	0.249	0.001		
Mean firm-specific wage premium	0.104	0.075	0.028	0.189	0.125	0.064	0.035	0.1%	99.9%
Percentiles of firm-specific wage premium									
25th	0.030	0.009	0.021	0.100	0.041	0.059	0.038	15.8%	84.2%
50th	0.125	0.089	0.035	0.216	0.133	0.083	0.048	-5.7%	105.7%
75th	0.191	0.170	0.021	0.299	0.238	0.061	0.040	-2.6%	102.6%
Panel C. An alternative counterfactual using pooled AKM Models									
Mean log real daily wage			0.247			0.247			
Mean firm-specific wage premium	0.129	0.079	0.050	0.180	0.127	0.053	0.004	11.0%	89.0%
Share of row 1			*(20.1)*			*(21.7)*	*(1.6)*		

Note: Table shows decomposition results of reweighting the distribution of male and female firm premiums to the sorting and bargaining effect distributions of the 1995-2001 period. In panel A, small differences in displayed statistics relative to Table 2.3 occur because, for the decomposition exercise, I omitted the year 2001 from the first period (1995-2001) to obtain a standard DFL decomposition (the AKM firm effects and the normalisation are still based on 1995-2001). This also impacts on the 2001-2008 period due to differences in the relative weight of each observation in the total sample. In panel B, I assign men and women in the 2001-2008 period their corresponding position in the sorting and bargaining effect distribution in the 1995-2001 period. The reweighting is based on deciles of sorting and bargaining effects, as described in the main text. Panel C uses the normalisation of c=3.15 which was obtained by the algorithm described in the main text.
Source: LIAB Mover Model 9308

the rise in firm gender disparities over time, then the fraction of the change explained by reweighting should be small and the fraction attributable to returns should be large. Indeed, as the last two columns reveal, virtually the entire rise in firm gender differentials is driven by the return structure.

An alternative way to assess whether the observed expansion is driven by changes in returns or changes in employment is to simply use the normalised firm premiums from AKM models that pool both periods into one estimation per gender (Table A.5), thus eliminating changes in the return distribution. As shown in panel C, this approach yields similar results regarding the driver of firm gender disparities, with roughly 90% (1-0.4/3.6) of the increase related to changes in the return distributions.

This analysis shows that changes in the firm-specific wage structures worked to the detriment of women by amplifying initial gender disparities both within and between firms, despite stable relative employment shares. More specifically, these changes placed women at an increasing disadvantage through i) higher wage firms employing relatively fewer women and featuring faster firm premium growth over time, and ii) male firm premiums rising faster than female premiums in the same firms. Next, I show that these changes in the firm wage structure also explain why gender gaps stalled within narrowly defined occupations and industries despite continuously converging gender shares (Figure 2.1).

Occupations and Industries: Female employment shares differ widely across occupations, ranging from 7.9% in crafts to 64.9% in services and sales.[27] If firms employ different shares of male- or female-dominated occupations, the decompositions may confound shifts in gender-specific rents with changes in returns to occupations or occupational employment shares. On the other hand, if firm premiums expand even within occupations, this could explain why Figure 2.1 shows a stagnating gender gap within narrowly defined occupations despite continuously declining segregation.[28] When I recompute the decomposition for seven major occupation groups, I find a strong variation in firm gender gaps, ranging from 0.014 for plant and machine operators to 0.103 for senior officials and managers (Table A.10, col. 5-8). While sorting effects dominate in all groups, I discover notable bargaining effects in occupations related to crafts and services/sales (4.1% and 1.9% of the total wage gap). Figure 2.4, panel A, plots the associated *changes* in sorting and bargaining effects for each occupation group against female employment shares in the 1990s. I find the largest rise in sorting effects for professionals and managerial occupations, i.e., high-powered and male-dominated jobs that are concentrated in higher wage firms to begin with. On the other hand, bargaining effects are expanding the most in service/sales occupations. The linear fits suggest that both effects tend to rise faster in female-dominated occupations, though the net effect on

[27] Calculations refer to nine ISCO Major Groups where I assign each worker his or her modal occupation in a period, and lump together groups 1 (elementary) and 2 (agricultural) as well as 8 (professionals) and 9 (senior officials and managers).

[28] To quantify occupational segregation, I use the Duncan index (Duncan and Duncan, 1955) for three-digit occupations. This measure gives the percentage of women (men) that would have to change jobs to equalise the occupational distribution between genders. It declined from about 0.6 to 0.55 between 1995 and 2008, though at a slower pace in more recent years.

firm gender inequality is mitigated by slowly declining occupational segregation.

In panel B, I conduct a similar exercise for 16 major industries, with associated decompositions reported in Table A.11. As the table illustrates, the net effect of firm premiums varies from 0.104 in the building sector, with a bargaining effect of 0.094, to -0.042 in public administration, with a bargaining effect of -0.046.[29] Moving to panel B of Figure 2.4 shows a strong variation in the changes across industries over time. Perhaps the most interesting observation is a substantial rise of the bargaining effect by 0.046 (15% of the 1990s gender wage gap) in the automotive industry. This sector has witnessed substantial globalisation pressure and wage flexibilisation in the recent past, adumbrating a potential link between growing firm gender gaps and changes in the institutional environment (section 2.4.5).

2.4.4 Evidence on Gender Disparities in Rent-Sharing

The negative or zero bargaining effect from the baseline decompositions (Table 2.3) is hard to reconcile with a notion of women bargaining less effectively or being less likely to initiate wage negotiations. It is conceivable, though, that the firm premium-based measure of bargaining is contaminated by invariant firm characteristics that affect male and female wage setting differently. In this section, I present several pieces of evidence consistent with a female disadvantage in wage negotiations, and conclude that the equalising effect of institutionalised wage setting may represent one such source of contamination.

Estimates From Between-Firm Mobility: A simple way of gauging the relative rent-sharing elasticity is to compare the wage changes of men and women who move between jobs associated with different wage levels. To this end, I replicate the event study of CCK based on transitions between coworker wage quartiles in each period, and regress the covariate-adjusted three-year wage change of female movers on the corresponding changes of male movers (Figure A.10).[30] I find a slope of around 0.9 (SE=0.026) in each period, suggesting that women gain (or lose) about 90% of the associated male wage change when moving between firms with higher (or lower) paid coworkers.

The previous estimates could be affected by the relative distributions across firms, so I next examine gender differentials in the same firms. As shown in Table 2.2, the correlations between $\hat{\psi}_j^M$ and $\hat{\psi}_j^F$ are 0.57 and 0.67, and the associated regressions of female on male firm premiums yield slopes of 0.73 and 0.79. These estimates of the relative rent-sharing elasticities are presumably downward biased due to sampling error in $\hat{\psi}_j^M$ and $\hat{\psi}_j^F$ (CCK). To address this, Figure 2.5 plots the normalised female premiums against male premiums, grouping firms into 50 percentiles of value added and averaging premiums within each bin.[31]

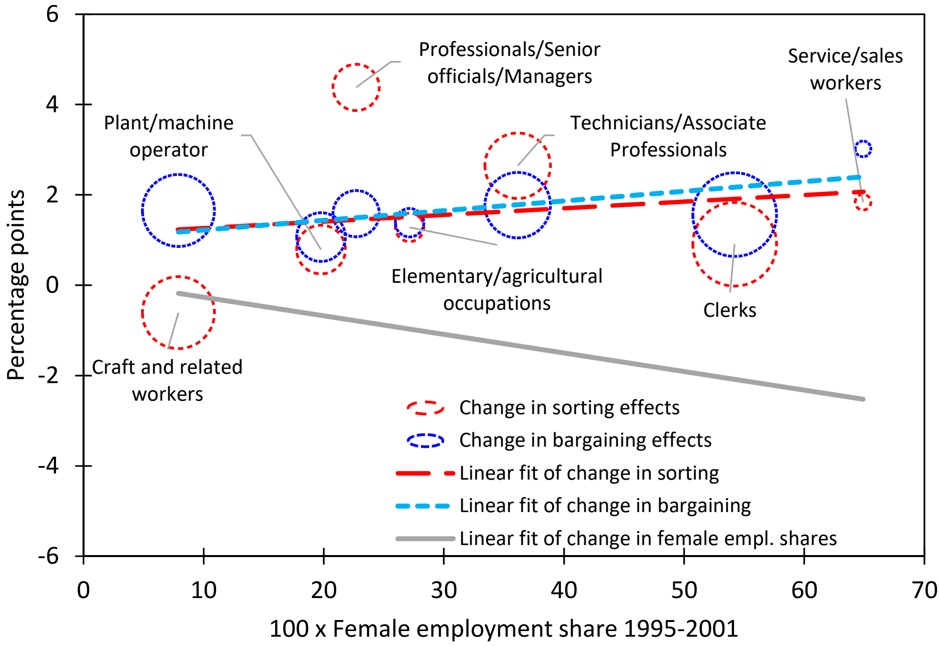
[29] One may be concerned that the overall negative bargaining effect is a result of this particular sector. To evaluate this, I dropped this sector from the sample, re-estimated the AKM models, and went through the entire post-estimation analysis. The decomposition results were quantitatively and qualitatively similar.

[30] The covariate adjustment is based on models for stayers with similar education and experience; for details, see section A.4 of the Appendix and CCK. Complete results of the event study are summarised in Tables A.6 and A.7.

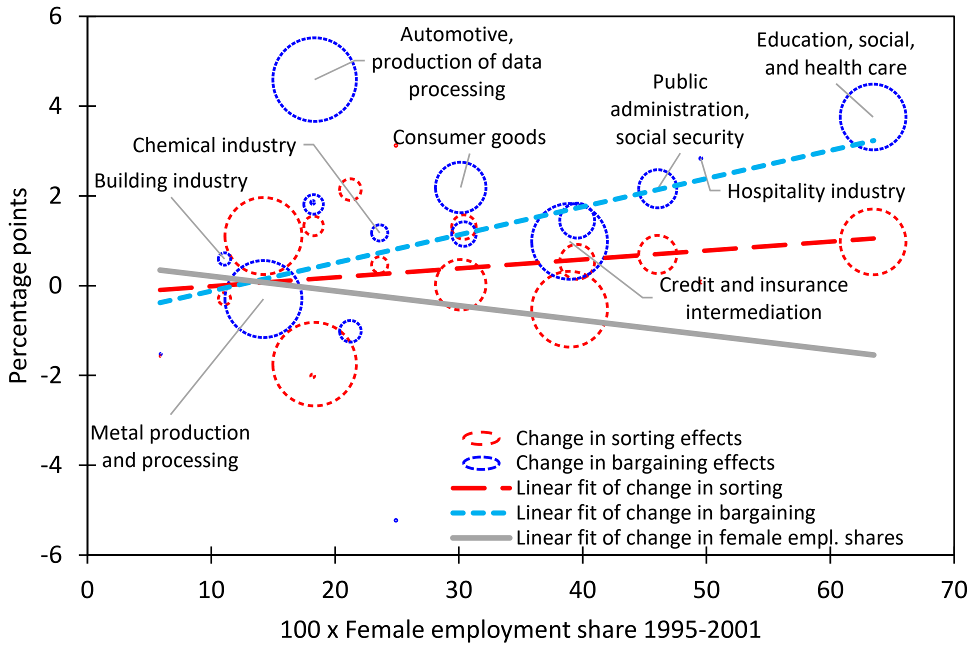
[31] Since each bin contains roughly similar numbers person-years, this approach is comparable to a two-stage

Figure 2.4: Changes in Sorting and Bargaining Components and Changes in Female Employment Shares Across Occupations and Industries

A. 7 Major Occupations

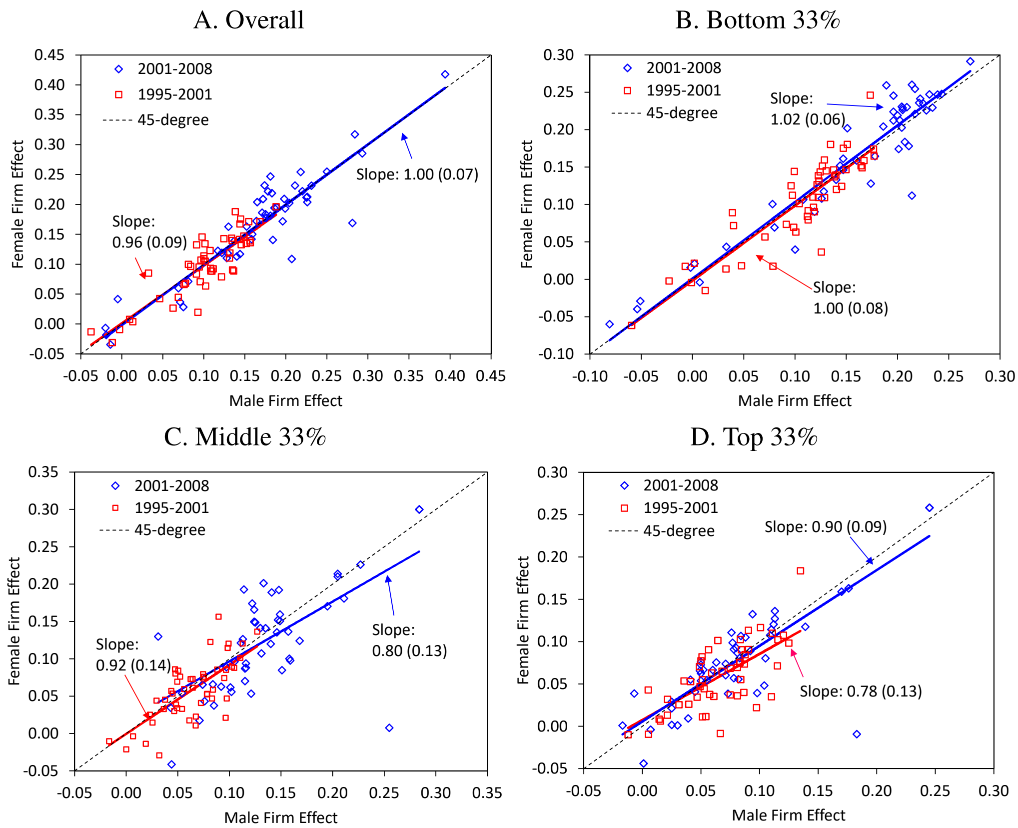


B. 16 Major Industries



Note: Figures plot the cross-period changes of sorting and bargaining effects obtained from decompositions of firm premiums computed within industries and occupations. In the case of occupations, I assigned each worker his or her modal occupation in a given period. Each group is associated with two bubbles (although I only display labels for one bubble for clarity), one for the change in sorting (red) and one for the change in bargaining (blue), where the size of the bubble is proportional to that group's share in total employment in 1995-2001. In panel B, I do not show all value labels for clarity. Occupations and industries are ranked by the female employment shares (person-year weighted) in the 1990s. The grey line depicts a linear fit of regressions of the change in female employment shares against the initial share.
Source: LIAB Mover Model 9308

Figure 2.5: Normalised Male and Female Firm Effects Across the Distribution of Wages Grouped by Mean Log Value Added per Worker in the 1990s and 2000s



Note: Figure plots the normalised female firm premiums against the associated normalised male firm premiums, grouped into 50 bins of mean log value added per worker for the 1990s (red squares) and 2000s (blue diamonds). The associated fitted relationships are shown in corresponding colours. Slope coefficients and standard errors refer to the bins above average zero surplus firms. The grey line indicates the 45-degree slope.
Source: LIAB Mover Model 9308

Focusing on panel A, the fitted relationships in each period yield a slope near 1, suggesting no rent-sharing differentials between genders. Breaking out this relationship by tertiles of the wage distribution (panels B-D) reveals that this patterns is driven by the least paid workers. Looking at the top two thirds of the distributions, I find that women receive only about 80-90% of male firm rents.

The lack of a bargaining effect in the lower tail of the wage distribution points to a mediating role of institutions, and in particular, collective bargaining. Collective agreements extend to the majority of workers, and, by specifying wage floors at the industry/region or the firm level, tend to lift the bottom of the wage distribution relative to the rest. In a third approach, I therefore estimate a new set of AKM models (Table A.12) for a subgroup of industries with union coverage below the median value of 2010 (StaBu, 2013). I then repeat the normalisation and compute the decomposition in eq. (2.2). This exercise generates a relative rent-sharing coefficient of about 0.6 — even smaller than the 0.9 estimate obtained

least squares regression of female firm premiums on male premiums instrumented using log value added.

above and consistent with a negative impact of unions on the bargaining channel.[32]

Estimates From Within-Firm Variation: Another approach to gauge the relative rent-sharing elasticity of women is to use within-firm variation in productivity and wages over time (see CCK for details). By focusing on job stayers, this design purges rent-sharing elasticities from time invariant characteristics with potentially heterogeneous impacts on male and female wages, and can thus be viewed as an indirect test of the hypothesis that the bargaining effect in eq. (2.2) is confounded by institutionalised wage setting. It also constitutes the most conclusive investigation of male-female bargaining differentials.

To implement the stayers' design, I focus on a subsample of roughly 226,000 men and 60,000 women who stay with their employer for at least four consecutive years during the 1995-2008 period, and whose employer has non-missing information on log value added in all four years (Table A.4). I estimate gender-specific rent-sharing elasticities from regressions of the three-year wage change of male and female stayers on the associated change in excess log value added per worker.[33] An important concern in models relying on short run fluctuations in productivity is a potential attenuation bias caused by the large variability in productivity (Appendix A.2). To test the sensitivity of my estimates, I therefore impose different restrictions (trimming/winsorizing) on the associated three-year changes.

The main results are summarised in panel A of Table A.4. As in CCK, I first report male and female rent-sharing coefficients, and then their ratio, which I obtain from a two-stage regression of female wage changes on male changes, instrumented using excess log value added. My preferred model in row 1 yields rent-sharing elasticities of 0.024 for men and 0.016 for women, with an estimated ratio of 0.7 allowing me to reject the null of equal rent-sharing. These values are robust to alternative restrictions on productivity (winsorizing at +/-0.75 or +/-0.5), using value added rather than excess value added, and including industry and federal state fixed effects (col. 5-7). Row 3 reports coefficients from unrestricted productivity measures, which are (as expected) considerably smaller. Reassuringly, their ratio is again close to 0.7, though I cannot reject the null of equal rent-sharing.

To further test the sensitivity of these results, I use sales per worker as an alternative (perhaps less noisy) proxy of firm surplus, with the added advantage of relying on a 25% larger sample.[34] As summarised in panel B, I find that male and female rent-sharing coefficients rise by a factor of 2, with a somewhat lesser increase among men, which in turn pushes up the rent-sharing ratio to around 0.9.

[32]Based on the 2001-2008 sample, the bargaining effect in this exercise is 0.06, compared to 0.001 in Table 2.3. The average rent of male workers is 16.1%. If sorting effects were zero, then the average bargaining effect is given by 1-0.06/0.161.

[33]I estimate regressions at the firm level. To calculate the wage change, I first compute the residual three-year change from individual level regressions on a quadratic polynomial in age, and then average this residual at the firm level. Excess log value added (EVA) for firm j is computed as $\max\{0, VA_{jt} - \hat{c}\}$, where $\hat{c} = 3.15$ in the period-pooled sample.

[34]Value added per worker is computed from a combination of two survey variables, sales per year and the cost share of inputs, both of which are subject to measurement error. Certain data regularities in the cost share of inputs suggest that the measurement error problem may be even larger in that variable than in sales (Addison et al., 2003).

Table 2.5: Rent-Sharing Models for Male and Female 3-Year Stayers Between 1995 and 2008

	Nb. of estab's	Basic Model: Rent-sharing coefficients Male	Basic Model: Rent-sharing coefficients Female	Basic Model: Ratio M/F	Extended Model: Rent-sharing coefficients Male	Extended Model: Rent-sharing coefficients Female	Extended Model: Ratio M/F
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Excess log value added per worker, 1995-2008, three-year stayers							
Three-year change, trimmed at +/- 0.75	2,190	0.024	0.016	0.668	0.020	0.013	0.638
		(0.003)	(0.004)	(0.162)	(0.003)	(0.004)	(0.193)
Three-year change, Winsorized at +/- 0.50	2,371	0.021	0.015	0.699	0.019	0.013	0.690
		(0.003)	(0.005)	(0.176)	(0.003)	(0.004)	(0.205)
No restrictions	2,371	0.009	0.006	0.696	0.008	0.005	0.694
		(0.003)	(0.003)	(0.248)	(0.002)	(0.003)	(0.286)
Mean of exc. log VA		1.257					
Std. dev. of exc. log VA		0.614					
Lester Range (based on row 1)		0.046	0.031				
Panel B. Log sales per worker, 1995-2008, three-year stayers							
Three-year change, trimmed at +/- 0.75	2,948	0.046	0.044	0.937	0.039	0.035	0.881
		(0.004)	(0.006)	(0.105)	(0.003)	(0.005)	(0.122)
Three-year change Winsorized at +/- 0.50	3,007	0.047	0.046	0.985	0.040	0.037	0.930
		(0.004)	(0.006)	(0.112)	(0.004)	(0.005)	(0.128)
No restrictions	3,007	0.030	0.029	0.952	0.026	0.023	0.886
		(0.003)	(0.005)	(0.131)	(0.003)	(0.004)	(0.144)
Mean of exc. log sales		5.262					
Std. dev. of exc. log sales		0.708					
Lester Range (based on row 1)		0.025	0.023				

Note: Entries show coefficients of three-year wage changes of male and female stayers on three-year changes in log productivity. Wage changes are adjusted for a quadratic polynomial in age. In panel A, productivity is measured by value added per worker, in panel B by sales per worker. Three-year changes in productivity are adjusted as indicated in the row heading. The basic model (columns 2-4) includes year fixed effects, the extended model (columns 5-7) additionally includes industry (17 categories) and federal state (11 categories) fixed effects. Ratios in columns 4 and 7 are obtained via two-stage least squares, instrumenting the male firm effect by log productivity. All models are estimated at the establishment-year level, weighted by the total number of person-years (male+female) in the base year. Standard errors are clustered at the establishment level.
Source: LIAB Mover Model 9308

In sum, I conclude that female wages are about 80-90% as responsive to changes in productivity as male wages. This result contrasts with the decomposition based bargaining channel and suggests that the latter might be confounded by mechanisms other than individual rent-sharing between workers and firms. I will next scrutinise the role of collective agreements as one possible explanation.

2.4.5 The Decline of Collective Bargaining and the Rise in Rent-Sharing

In this section, I investigate how collective agreements mediate the relationship between productivity and wages, and evaluate its impact on firm gender differentials. Traditionally, collective bargaining in Germany takes place between employer associations and unions at the level of industries and regions, suggesting a relatively low association between firm-specific productivity and the wage premiums that firms pay. Over the 1990s and 2000s, this system became increasingly "localised", with opening clauses permeating and many old and new firms deciding to set wages outside collective agreements (Fitzenberger et al., 2008). As a result, wage setting became considerably more flexible through both a sharp decline of union coverage and greater decentralisation of wage setting inside the covered sector. These trends are widely viewed as an important source of rising wage and firm premium inequality (Dustmann et al., 2014; CHK), and by changing the structure of firm-specific wages potentially influence the firm premium gap between genders.[35]

Union Coverage, Wage Premiums, and Productivity: The developments described above suggest an increasing association between firm level wages and productivity over time, and they propose two mechanisms that engender this rise: one is a shift of workers from the covered to the non-covered sector; and the other is an increasing association between wages and productivity within the covered sector. *A priori*, both factors might be at play: first, since rent-sharing is higher in the non-covered sector, a shift from the covered to the non-covered sector generates a rise in *average* rent-sharing elasticities. Second, a poor economic performance in the 1990s and increased globalisation pressure might have diverted unions' objectives away from insurance against wage fluctuations towards protection against job losses, accepting higher rent-sharing inside the covered sector in return. To explore this, I first document a rise in average rent-sharing elasticities over time (pooling genders and coverage status), and then show that this rise is dominated by substantial adjustments inside the covered sector.

To commence, note that the results in Figure 2.3 and Table 2.3 already indicate that, on average, rent-sharing elasticities increased over time: for example, the fitted relationships in Figure 2.3 reveal a rise from 0.10 to 0.15 between the 1990s and 2000s, and the decompositions suggest that the average rent level rose from about 7.5-10.3% in the 1990s to 11.2-17.6% in the 2000s. For a more comprehensive analysis, and to bridge the gap to the recent rent-sharing literature, I decompose the wage elasticities with respect to mean log value added in each period into three components, measuring the contribution of worker skills (person effects + covariates) and firm premiums from gender-pooled AKM models (Table A.5). While the shares attributable to worker skills can be interpreted as sorting effects, the firm premium component provides a clean measure of rent-sharing as it is identified from excess variation between wages and productivity after worker skills have been remunerated. The results for three specifications are reported in Table A.13. The first model includes basic controls for human capital, and the second and third model additionally include state fixed effects plus either 16 or 254 industry dummies (compare Card et al., 2016 [CCHK], Table 4).[36] The main finding from this exercise is that average rent-sharing elasticities rise sharply

[35]The role of unions for gender inequality is analysed in Blau and Kahn (1996, 2003), and Antonczyk et al. (2010).

[36]Adding fixed effects for ten firm size groups leads to qualitatively and quantitatively similar results.

from 0.089 to 0.128 over time (row 3).[37] Similar patterns, though at a lower level, emerge within narrowly defined industries with elasticities still rising by about 10% across periods. These findings are consistent with a decentralisation of wage setting both within unions and through deunionsation as described above.[38]

Next, I examine how rent-sharing changed separately in the covered and the non-covered sector. To this end, Figure 2.6 breaks out the relationship from Figure 2.3 by contract status, assigning firm level agreements to union coverage. Although the estimates for the non-covered sector are relatively noisy (in the 1990s only about 10% of workers fall into this group), several striking conclusions emerge. Most importantly, the figure documents a sharp rise in rent-sharing elasticities *inside* the covered sector, suggesting that the trend increase in rent-sharing (Table A.13) results from a closer alignment between wages and productivity built into collective agreements. However, the patterns bespeak further changes in the structure of rent-sharing between the 1990s and 2000s. In particular, note that the vertical difference between average firm premiums of unionised and non-unionised zero surplus firms decreases from 12 to about 3-5 log points over time, suggesting that unions released upward pressure on wages in the least productive/low wage firms. This convergence is compensated by a substantial left-shift of the threshold above which rent-sharing starts. One interpretation of these patterns is that unions put up with smaller baseline premiums in exchange for substantially earlier and larger participation in productivity gains.

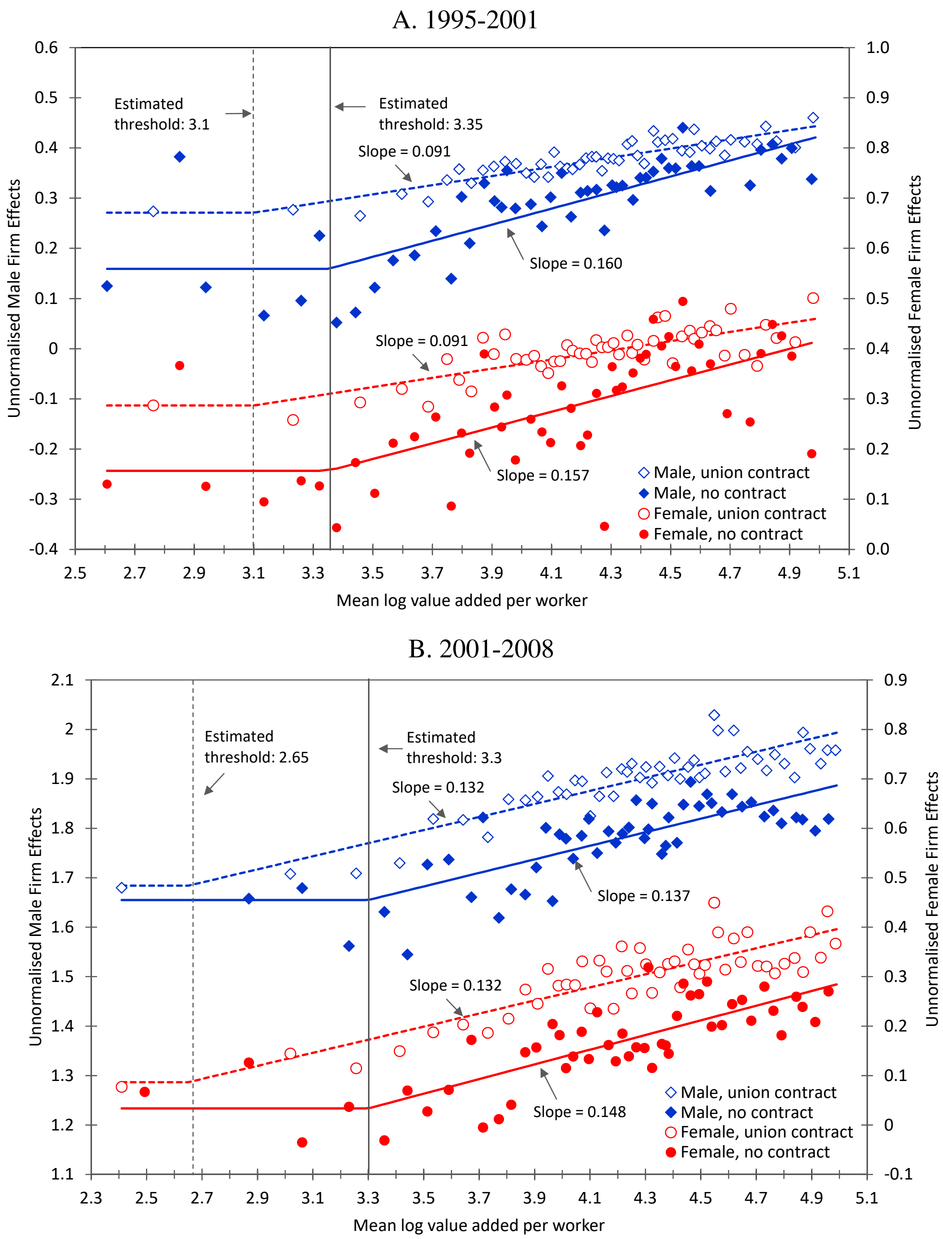
CHK document that workplace heterogeneity drives the change in overall inequality, and argue that, by means of new firms not entering and old firms leaving collective agreements, deunionisation contributes to this development. On the other hand, Dustmann et al. (2014) show that changes in overall wage inequality are largely driven by changes *inside* the covered sector, and this would be difficult to explain by growing workplace heterogeneity if the latter was related to cross-sector shifts alone. My findings offer an explanation for growing firm premium dispersion inside the covered sector: an expansion in rent-sharing coupled with growing productivity differentials over time (Figure 2.2).

Union Coverage and Firm Gender Gaps: The previous findings point to an indirect effect of unions on the gender firm premium gap, propagated through the structure of firm-specific wages. But they also point to an important role of changes in the relative distributions of union coverage between genders and the associated returns. To see this, it is useful to rewrite eq. (2.2) as a weighted average of firm premium gaps at unionised (U=1) and non-unionised

[37] Note that if I use *excess* mean log value added instead of overall mean log value added, the coefficients correspond to the average of male and female rent-sharing elasticities shown in Figure 2.3. In either case, the elasticities are larger than rent-sharing elasticities based on within firm variation. CCHK discuss several possible explanations for this pattern.

[38] Another remarkable regularity across all models is that around 40% of gross wage elasticities reflect sorting on ability, very similar to estimates for Portugal (CCHK).

Figure 2.6: Male and Female Firm Effects by Contract Status Grouped into 50 Bins of Mean Log Value Added per Worker



Note: Figure shows male (diamonds) and female (circles) firm premiums, grouped into 50 bins of mean log value added per worker separately for union covered (white filling) and non-covered (coloured filling) establishments in each period. Fitted relationships are estimated at the firm level separately by contract status. Mean log value added is trimmed above 5. The kinkpoint is estimated using the procedure described in the main text.
Source: LIAB Mover Model 9308

(U=0) firms in period p (Appendix A.9.1):[39]

$$FGG_p = E\left[\hat{\psi}_p^M|M_p,U_p\right] - E\left[\hat{\psi}_p^F|F_p,U_p\right] = FGG_p^{nu} + \left(s_{M,p}^u - s_{F,p}^u\right)UP_p^F + s_{M,p}^u\left(FGG_p^u - FGG_p^{nu}\right) \qquad (2.4)$$

[39]I maintain the convention of presenting decompositions in terms of male distributions and female returns.

Table 2.6: Developments in the Union Wage Premium and Union Coverage of Men and Women

	1995-2001		2001-2008		Δ	
	Sector level	Firm level	Sector level	Firm level	Sector level	Firm level
	(1)	(2)	(3)	(4)	(5)=(3)-(1)	(6)=(4)-(2)
Panel A. Estimates of the union wage premium (omitted category: no coverage)						
Male Firm-Effects (ψ^M)	0.084	0.054	0.128	0.091	0.044	0.037
	(0.008)	(0.012)	(0.011)	(0.017)		
Female Firm-Effects (ψ^F)	0.105	0.087	0.146	0.122	0.041	0.035
	(0.011)	(0.016)	(0.013)	(0.016)		
Gender gap in union wage premiums	-0.021	-0.033	-0.018	-0.031	0.003	0.002
	(0.008)	(0.011)	(0.007)	(0.014)		
# of person-years	2,757,239		4,405,641			
# of firms	3,204		4,676			
Panel B: Union coverage rates by gender (in percent)						
Men	86.9	8.7	71.7	12.0	-15.2	3.4
Total collective bargaining coverage	95.5		83.7		-11.9	
Women	85.9	7.8	69.2	10.0	-16.7	2.2
Total collective bargaining coverage	93.7		79.2		-14.5	
Male-female difference	0.9	0.9	2.4	2.0	1.5	1.2
Total collective bargaining coverage	1.8		4.4		2.6	

Note: Panel A shows coefficients from regressions of the variable indicated in the row heading on dummies for sector and firm level bargaining. The omitted group is "no collective bargaining". Models are estimated at the firm level (including all firms with at least one year of non-missing union status in the EP), separately for each gender, and weighting each regression by the total number of person-years in the dual-connected set in the corresponding period. Results are similar if male- and female-specific weights are used instead. Entries in columns 5 and 6 are differences across period. Panel B shows union coverage rates in 1995 and 2008 for each gender (note that I use point in time values here rather than averages across the entire periods). Values in italics correspond to the sum of the two entries in the row above. Standard errors in parentheses.
Source: LIAB Mover Model 9308

The first term in eq. (2.4) measures the gender gap in firm premiums for non-unionised workers (*nu*), and the next two terms show how unions (*u*) alter this relationship. The second term represents male-female differences in union coverage shares. If male workers are relatively more likely to be unionised ($s^u_{M,p} > s^u_{F,p}$) and the union wage premium of women is positive ($UP^F_p > 0$), then the net effect on firm gender gaps is positive. To investigate this, Table 2.6 summarises union wage premiums (panel A) and coverage rates (panel B) separately by gender and period, distinguishing between industry and firm level contracts.[40] As shown in the last row of panel B, the difference between $s^u_{M,p}$ and $s^u_{F,p}$ is in the order of 1.8-4.4 percentage points. Moreover, I find uniformly positive union wage premiums in the order of 5.4-14.6 log points, depending on the period and contract type.[41] Jointly, these

[40] To estimate union wage premiums, I regress male and female firm premiums on dummies for industry and firm level bargaining for each subsample. The models are estimated at the firm level, weighting each firm by the total number of person-years. Union status is a highly persistent firm characteristic, hence, union returns load into the firm-effects of eq. (2.1). In cases where union status changes, I assign the modal coverage type to all years a firm is observed. I split ties in favour of the stricter agreement, thus giving priority to industry-level bargaining over firm-level bargaining over no coverage. This is motivated by the 'Nachwirkungsfrist', which limits an employer's ability to adjust wages of old employees in the short-run.

[41] These estimates are in the same ballpark as those presented in CHK for the sample of male workers, and are robust to the inclusion of birth cohort dummies (3-year bins).

patterns suggest that unions indeed expand gender disparities through differential coverage probabilities.

The third term gauges the relative impact of unions on the firm premiums of each gender. If women gain more from unionisation than men, e.g., because collective agreements enforce relatively higher gender pay equality, then within firm gender gaps are smaller in covered firms, $FGG_p^u < FGG_p^{nu}$, corroborating that unions might rationalise the negative bargaining effect noted in Table 2.3. Indeed, as shown in panel A of Table 2.6, a regression of $\widehat{\psi}_j^M - \widehat{\psi}_j^F$ on union dummies suggests that unions compress gender inequalities by about 1.8-3.3 log points, sufficiently large to explain the patterns in Table 2.3.

Deunionisation and Changes in Firm Gender Gaps: The period-specific estimates show that unions alter the gender wage gap by affecting the average returns to men and women. To guide the analysis of the impact of *de*unionisation, I rewrite eq. (2.4) in terms of changes (Appendix A.9.1):

$$\begin{aligned} \Delta FGG =& \Delta FGG^{nu} + \left(s_{M,1}^u - s_{F,1}^u\right) \Delta UP^F + \left(FGG_1^u - FGG_1^{nu}\right) \Delta s_M^u. \\ &+ UP_2^F \Delta \left(s_M^u - s_F^u\right) + s_{M,2}^u \Delta \left(FGG^u - FGG^{nu}\right) \end{aligned} \tag{2.5}$$

where Δ denotes changes across periods (1: 1990s; 2: 2000s). The three terms in the first line summarise the impact of deunionisation that arises through shifts in union premiums of women (ΔUP^F), changes in union coverage rates of men (Δs_M^u), or through changes in the firm gender gap for non-unionised workers (ΔFGG^{nu}). These components depend only on initial gender differentials, and show that even if the decline of unions was not associated with relative (male-female) shifts in the coverage shares or union premiums, there would still be an effect on firm gender differentials arising from deunionisation. Table 2.6 illustrates that union premiums of women rose by 3.5-4.1 log points over time ($\Delta UP^F > 0$), whereas union coverage of men declined by 11.9 percentage points ($s_M^u < 0$). Both developments increase the firm gender gap because men are initially more likely to be unionised, $s_{M,1}^u > s_{F,1}^u$, while the firm gender gap for unionised workers is compressed, $FGG_1^u < FGG_1^{nu}$. In addition to these general shifts, the two terms in the second line gauge the effect of *relative* changes in union coverage rates and union premiums between men and women. Inspection of Table 2.6 again suggests a positive effect on ΔFGG: women were about 2.6 percentage points more likely to leave union coverage, and the gender gap in union premiums expanded by some 0.2-0.3 log points.

To provide a more comprehensive summary of the impact of deunionisation, I estimate counterfactual firm gender gaps using a DFL reweighting approach. In a first exercise, I assign men and women in 2008 the gender-specific contract distributions of 1995 to gauge how much of the rise in firm gender inequality is associated with the overall decline of union coverage. In a second exercise, I assign women the period-specific male contract distributions to estimate the related contribution of the gender bias in deunionisation. The unexplained share in these decompositions is a composite of changes in the union/non-union

Table 2.7: The Role of Deunionisation for the Rise in Gender Inequality

	1995			2008			Period change in	Perc. of (7) due to
	Male	Female	Δ M-F	Male	Female	Δ M-F	Δ M-F	reweighting
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Observed								
Mean log real daily wage	4.613	4.355	0.258	4.598	4.343	0.256	-0.003	
Mean firm premium	0.126	0.105	0.021	0.168	0.104	0.064	0.043	
Percent of total gender wage gap			(8.2)			(25.0)	(16.5)	
Percentiles of firm premium								
25th	0.069	0.041	0.028	0.076	0.014	0.062	0.033	
50th	0.144	0.106	0.038	0.197	0.114	0.083	0.045	
75th	0.191	0.186	0.005	0.293	0.222	0.071	0.066	
Panel B. Assign gender-specific contract distributions from 1995 (weighted by 2008 returns)								
Mean log real daily wage	4.613	4.355	0.258	4.611	4.377	0.234	-0.024	
Mean firm premium	0.126	0.105	0.021	0.167	0.115	0.052	0.031	27.4
Percent of total gender wage gap			(8.2)			(22.3)	(12.0)	
Percentiles of firm premium								
25th	0.069	0.041	0.028	0.080	0.036	0.044	0.016	53.1
50th	0.144	0.106	0.038	0.181	0.117	0.064	0.026	42.5
75th	0.191	0.186	0.005	0.272	0.212	0.060	0.055	16.6
Panel C. Reweight period-specific contract distributions of men and women								
i. Assign women the contract distribution of men (weighted by female returns)								
Mean log real daily wage	4.613	4.356	0.257	4.598	4.351	0.248	-0.010	
Mean firm premium	0.126	0.106	0.020	0.168	0.108	0.060	0.040	7.4
Percent of total gender wage gap			(7.9)			(24.1)	(15.3)	
ii. Assign women the returns to union coverage of men (weighted by female contract distribution)								
Mean log real daily wage	4.614	4.355	0.259	4.593	4.343	0.251	-0.008	
Mean firm premium	0.125	0.105	0.021	0.163	0.104	0.059	0.039	9.3
Percent of total gender wage gap			(8.0)			(23.7)	(4.9)	

Note: The top panel shows observed values for the first and second period using the original period definition with overlap in 2001. The middle panel repeats the observed distribution in columns 1-3 using 1995-2000 instead of 1995-2001 as first period. Columns 4-6 then show the counterfactual distributions holding constant the gender-specific contract distribution and returns at their 1995-2000 values. The bottom panel again replicates the observed distribution of men (columns 1 and 4), and displays counterfactual distributions for women in columns 2 and 5, where the counterfactual is computed by assigning women the contract distribution of men. Column 7 shows the change of the gender gap in each panel, whereas columns 8 and 9 calculate the percent share of the decomposition components relative to the observed overall change (top panel).
Source: LIAB Mover Model 9308

return structure that includes, in particular, the rise in rent-sharing documented above. This suggests that the (partial) impact of deunionisation is a lower bound for the overall effect of unions on firm gender gaps since the latter includes the indirect effect that emerges through a widening of the firm premium distribution by means of increased rent-sharing in the covered sector.

Table 2.7 reports the results of these decompositions. The entries in panel A refer to the observed distributions of gross wages and firm premiums in 1995 and 2008.[42] In the first two rows, I replicate the mean growth in firm gender disparities, adjusted for the modified reference years. The last three rows focus on bottom, middle, and top percentiles of the firm

[42]Unlike the rest of the analysis, I pick specific years rather than periods to obtain sharper results. Hence, there are some small deviations between the entries in panel A, rows 1 and 2, and the results reported in the rest of this paper.

premium distribution, and reveal that gender differentials in firm rents have been rising in the overall level of rents. In panel B, I display the results of reweighting the 2008 wage and firm premium distributions of men and women to their industry level and firm level coverage rates of 1995. Though it is hard to interpret the relationship as causal, the entries in column 7 suggest that in the absence of (gender-specific) deunionisation, the overall gender gap would have narrowed by 2.4 log points instead of an observed 0.3 log point convergence (panel A, row 1). Moving to row 2, I find that around half of that 2.1 log point difference is attributable to a smaller rise of the firm gender gap. In particular, holding gender-specific unionisation rates constant at their 1995 levels, the firm gender gap would have expanded by only 3.1 log points, suggesting that deunionisation accounts for about 27% of the rise in firm gender inequality. Comparisons across the distribution (rows 3-5) reveal that the impact of deunionisation is greatest in the lower tail of the distribution, where it accounts for more than 50% of the rise in firm gender differentials, and is fading towards the upper tail, where it explains less than 20% — very similar patterns can be found in the wage inequality literature (e.g., Dustmann et al., 2009). Overall, the results suggest that, by permitting firm gender disparities to expand, deunionisation contributes to the stagnation of the gender wage gap since the 1990s.

Next, I turn to gauge the effect of higher deunionisation rates among women. To do so, I assign women the male unionisation rates in each period, which essentially imputes the male *de*unionisation rate to women (vice versa).[43] Panel C.i shows that eliminating the gender bias in deunionisation reduces the firm gender gap in row 2 by 0.3 log points (4.0-4.3), or 7.4%. As the associated growth in the overall gender wage gap (row 1) would have been 0.7 log points smaller under this counterfactual than the actual change in panel A (0.3 log points), these figures suggest around 40% (0.3/0.7) of the gender bias effect in male-female wage gaps, and roughly 15% (0.3/2.1) of the overall deunionisation effect (panel B, row 1) is due to the larger union drop-out probability of women. As shown in panel C.ii, the results are quite similar when I reverse the reference group, assigning women the male returns to coverage in each period. Altogether, this last decomposition exercise suggests that relatively higher deunionisation rates of women between 1995 and 2008 contribute to the observed expansion of firm gender inequality. At the same time, however, my results underscore the important role of differences in union/non-union wage premiums for men and women as well as their changes over time (panel A, Table 2.6).

2.4.6 Age Profiles and the Impact of Childbirth

It is a well-established fact that the gender wage gap is rising over the age profile, and more recent studies document that the firm plays a crucial role in this development (Barth et al., 2017; CCK). My analysis suggests that this role gains importance as firm heterogeneity expands over time, stressing the need to understand *why* firm premium differentials are widening in age to begin with. In a final step of my analysis, I use my measure of firm gender

[43] As shown in section A.9.1 of the Appendix, this approach holds only approximately.

inequality to investigate the impact of childbirth on gender gaps over the lifecycle and the role that firm premiums play in this.

To motivate, Figure 2.7 illustrates how the gender wage gap changes in age, and how firm-specific wages contribute to this in the 2000s (compare Figure VI in CCK). From the mid 20s to mid 40s, the overall wage gap widens dramatically by 22.6 log points (1.1 log points p.a.), with rising firm premium differentials contributing some 25-30% to this change. This is generated by, firstly, faster growth of male firm premiums until the mid 30s, and secondly, declining female firm premiums between the mid 30s and mid 40s. Both male and female average firm premiums are driven almost entirely by sorting differentials, meaning that men progress faster than women to more productive firms over their career. Crucially, and in line with growing firm premium differentials over time, examining the same relationship for the 1990s indicates a total firm premium contribution of only 10%.[44]

That rising firm premium inequalities in the 30s and 40s are driven by declining premiums among women suggests a link to work interruptions due to childbirth. More specifically, the birth of a child might slow down mothers' progression to higher wage firms, for example, by increasing the burden of job search and career investments due to higher costs of regional mobility.[45] Accordingly, Kleven et al. (2017) show for Denmark that childbirth is a key determinant of persistent gender disparities, and Barth et al. (2017) find that it is married women who drive the age dynamics between genders.

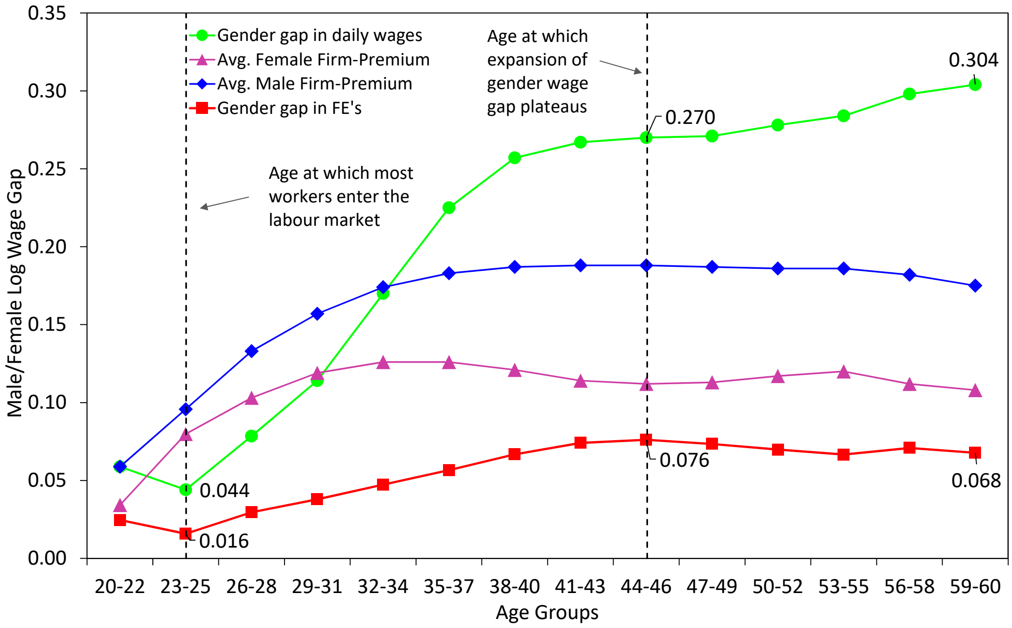
Though I do not directly observe the birth of a child in the data, I can draw on employment biographies with daily accuracy coupled with detailed information on the reason of work interruptions to identify temporary leave spells that are most likely caused by childbirth. The details of this procedure are described in section A.8.1 of the Appendix, but here I emphasise two aspects critical for the interpretation. First, in order to maintain consistency with the previous analysis, I limit attention to mothers working full-time before and after childbirth. This restriction potentially generates a selection bias since many mothers initially return to the labour market with reduced working hours. While beyond the scope of this paper, I acknowledge that integrating the part-time dimension is important to fully understand the career impacts of children on gender inequality. Second, my analysis is based on unique person-year observations, but childbirth events appear throughout a year. As a result, the estimated effects in the event year might be upward biased by pre-birth wages, suggesting that the interpretation should focus on longer run effects.

To estimate the impact of childbirth, I draw on the event study design of Kleven et al.

[44]Related to this, Barth, Goldin, Kerr, and Olivetti (2017) and Barth, Kerr, and Olivetti (2017) analyse gender earnings dynamics during the first 20 years after labour market entry, distinguishing within- and between-establishment contributions. They show that about 40% of the growing gender gap is attributable to a higher mobility of men into higher wage establishments. My estimates of the between-firm contribution are in a similar ballpark, though derived from a very different empirical approach.

[45]Childbirth might also alter women's valuation for job amenities, leading them to seek employment in firms that offer higher benefits, or to negotiate compensation packages with higher benefits in exchange for lower pay. However, this would warrant a different and less straightforward interpretation of the relative rent-sharing coefficients estimated in section 2.4.4. In particular, assigning the entire male-female bargaining gap to women's relatively lower bargaining power would no longer be suitable if women receive some of the higher rents available at more productive firms through greater amenities.

Figure 2.7: The Gender Wage Gap Over the Life Cycle and the Contribution of Firm Premiums: 2001-2008



Note: Figure shows gender gaps and mean firm premiums over the lifecycle of men and women between 2001 and 2008. The overall gender gap in log daily wages is plotted in green circles, the gender gap in firm premiums in red squares. The remaining two lines refer to the mean firm premiums of men and women.
Source: LIAB Mover Model 9308

(2017), adapted to the limitations of my sample (see Appendix A.8.2 for details). In particular, I identify a treatment group of some 270 thousand "actual mothers" and a control group of around 520 thousand "placebo mothers", and follow them around the birth of a first child between 1995 and 2008. The basic estimation equation has the form:

$$O^s_{itk} = \sum_{j \neq -1} \alpha^s_j \mathbb{I}\left[j = k\right] + \sum_h \beta^s_h \mathbb{I}\left[h = age_{it}\right] + \sum_y \gamma^s_y \mathbb{I}\left[y = t\right] + \eta^s_{ik} \tag{2.6}$$

where O^s_{itk} is the daily wage or firm-specific wage premium of woman i in year t at event time k, with $k = \{-5, \ldots, 13\}$ denoting years relative to childbirth.[46] I estimate the model separately for unbalanced panels of actual (s=a) and placebo (s=p) mothers, and include full sets of age- and year-dummies to control for different age profiles and macro shocks.[47] As childbirth effects are relatively large, I specify O^s_{itk} in levels, and calculate percentage effects by dividing each coefficient through the predicted outcome of group s when omitting the childbirth effect: $\hat{b}^s_k = \hat{\alpha}^s_k / E\left[\widetilde{O}^s_{itk}|k\right]$, where $\widetilde{O}^s_{itk} = \sum_h \hat{\beta}^s_h \mathbb{I}\left[h = age_{it}\right] + \sum_y \hat{\gamma}^s_y \mathbb{I}\left[y = t\right]$.[48] Finally, I define the motherhood penalty for outcome O at time k as the difference between coefficients of actual and placebo mothers: $P_k\left(O_k\right) \equiv \hat{b}^a_k - \hat{b}^p_k$.

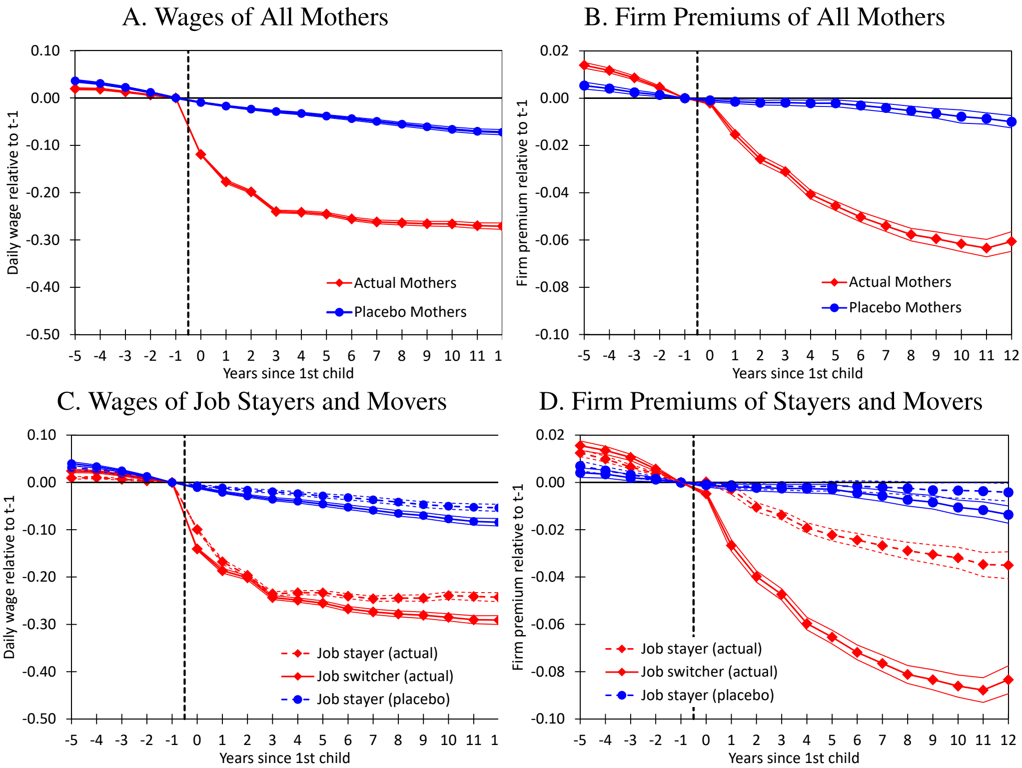
Figure 2.8 plots the impact of childbirth on wages and firm premiums for actual and

[46] I use the female firm effects from period-pooled models summarised in Table A.5.

[47] This allows me to analyse a longer time frame around birth with a sufficiently large sample size (the sample covers only 14 years of data). To address potential selectivity issues that may arise in this case, I also estimated models with individual fixed effects, which yielded broadly similar (but quantitatively larger) results.

[48] Estimates based on log wages are qualitatively similar and always in comparable magnitude.

Figure 2.8: The Wage and Firm Premium Impact of Children Around the Event of Childbirth Between 1995 and 2008



Note: Figure shows the wage and firm premium trends of actual (red/diamonds) and placebo (blue/circles) mothers around the event of childbirth. For a description of the sample, see main text and Appendix. All regressions control for full sets of year and age dummies, and are estimated in levels. Percentage changes are computed in a second step by dividing the estimated coefficients through the predicted value of the covariates excluding the impact of childbirth. Panels C and D distinguish between job stayers and job movers, identified through comparisons of the establishment ID before and after childbirth. Standard errors are clustered at the person-level.
Source: LIAB Mover Model 9308

placebo mothers across event years, measured relative to the year before childbirth (k=-1). Panel A shows that childbirth has an immediate (k=0) negative effect on wages of full-time mothers of around 11 log points, which nearly doubles in the three following years, and remains flat thereafter.[49] As shown in panel B, firm premiums contribute strongly to this effect: after childbirth, average firm premiums of mothers decline gradually for some 6 to 7 years, and then stabilise around 5-6 log points below the level of placebo mothers, accounting for 25-30% of the total motherhood wage penalty. These patterns reveal that a sizeable share of the persistent motherhood wage penalty is related to women sorting into firms that pay lower firm premiums. To better understand the decline of firm premiums after childbirth, I split the sample into mothers who change jobs after childbirth and those who stay with their

[49]The gradual decline may be related to timing issues and the fact that many mothers return to full-time employment only after some years passed (see above discussion). For example, during the observation period children usually entered preschool at age 3, enabling women to increase their working hours and possibly return to full-time employment.

Table 2.8: The Long Run Impact of Childbirth on Women's Wages and Firm Premiums

	All	Job Mobility		Skill Groups		
		Stayer	Mover	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)
Real daily wage	-0.197	-0.190	-0.204	-0.103	-0.196	-0.188
	(0.004)	(0.006)	(0.005)	(0.011)	(0.005)	(0.009)
Firm premium of females	-0.051	-0.030	-0.071	0.007	-0.051	-0.070
	(0.002)	(0.003)	(0.003)	(0.007)	(0.003)	(0.005)
Percent explained by firm premium	25.8	15.8	34.9	-6.7	26.1	37.2

Note: Table shows estimates of the long run impact of children on mothers' wages (row 1) and firm premiums (row 2) as well as their ratio (row 3). Column 1 shows estimates for the full sample of women, columns 2 and 3 distinguish between mothers who return to the same job and those who change jobs after returning to the labour market. Columns 4-6 show estimates for different skill groups. Estimates in rows 1 and 2 were obtained by calculating the difference in coefficients on the dummy for event year 12 (the motherhood wage penalty as defined in main text) from separate regressions for actual and placebo mothers. Standard errors are clustered at the person-level and calculated using a pairs bootstrap with 1,000 replications.
Source: LIAB Mover Model 9308

pre-birth employer.[50] As shown in panels C and D, firm premiums decline more than twice as fast among switching mothers, despite comparable overall wage losses.

Table 2.8 reports the long run wage (row 1) and firm premium (row 2) penalties for mothers, along with the share that is attributable to firm premiums (row 3). I focus on year 12 after childbirth and report the difference between the coefficient estimates of actual and placebo mothers.[51] Firm premiums explain around one quarter (25.8%) of the long run wage penalty, with a much larger share for the sample of movers (34.9%) compared to stayers (15.8%). The last set of columns breaks out the childbirth penalties by skill groups. Wage penalties for the high and medium skilled are around 8-9 log points larger than among the least skilled, and 60-85% of that difference is related to firm premiums.

So far, I focused on comparisons between mothers and non-mothers, but omitted that wage dynamics of non-mothers may not be representative for male workers. To bridge this gap, I investigate the gender gaps in wages and firm premiums over the lifecycle of actual and placebo mothers, fixing the number of years since first birth to ten. I use this restriction to focus on long run effects and to avoid confounding childbirth effects with age dynamics. The dynamic relationship is plotted in Figure A.15, and it shows that, despite a convergence of gender gaps for placebo mothers and actual mothers over the lifecycle, even at age 48, wage gaps between men and mothers are about 3.6 log points larger than between men and non-mothers, with half of that difference arising from firm premiums.

2.5 Conclusions

A stagnating gender wage gap and a pervasive rise in the dispersion of firm-specific wage premiums describe two characteristic developments of advanced labour markets in recent

[50] I identify job switches by comparing the last firm identifier before childbirth with the first firm identifier after childbirth.

[51] I report bootstrapped standard errors based on 1,000 replications, clustered at the person level.

decades. Using linked employer-employee data for Germany between 1995 and 2008, I show that firm premiums are an important determinant of male-female wage inequality, and document that a key source of the stalling progression of women over the past two decades is a widening in the dispersion of gender-specific firm premiums that placed women at an increasing disadvantage. In particular, I show that growing firm premium heterogeneity prevented the gender gap from narrowing by about 15%.

My analysis sheds light on the possible sources of these developments by examining the link between firm-specific wages and productivity, and embedding these developments into the specific institutional environment. In particular, I find that a growing decentralisation of wage setting plays a decisive role for the rise in firm gender disparities between the 1990s and 2000s. My analysis also highlights the consequences of these developments for several important dimensions of gender inequality such as occupations, industries, and age groups. Looking at age dynamics more specifically, I show that childbirth substantially slows down women's progression to higher wage firms over their career.

Overall, my analysis of changing firm premiums offers a solution to the puzzle of substantially slower and even stagnating gender convergence despite continuously narrowing skill supplies over the past two decades. In particular, while women make continuous headway along many of the classic dimensions of gender inequality, though in some cases at a slower rate, their progression is curbed by firm-specific wages growing increasingly unfavourable towards workers in lower wage firms.

3 The Impact of Immigrants on Native Wages and Employment: An Analysis of Refugee Inflows in the Early 1990s

3.1 Introduction

The considerable rise in immigration following the Balkan conflicts and the fall of the Iron curtain in the late 1980s triggered an unprecedented inflow of refugee migrants to West Germany, and led to significant and lasting changes in the composition of its population: between 1985 and 1995, the stock of immigrants rose by 2.8 million individuals, equivalent to a 64% rise in the initial foreign population and implying a total population growth of 5%.[1] About two decades later, an even larger influx of refugees — sourced in the "Arab Spring" (Dustman et al., 2016) — has put the topic of immigration back centre-stage in the political discourse in Germany and all other refugee-receiving countries across Europe, with right-wing anti-immigration parties successively gaining important long term political mandates.[2] Despite the significance of the topic and the availability of a historical blueprint, there is only limited empirical evidence on one core question nourishing public uncertainty and the political debate: What are the labour market effects of such a refugee-driven supply shock on the resident native population? Building on detailed administrative data for the West German labour market, we provide the first comprehensive answer to this question by analysing the short and long run effects of an unexpected refugee shock hitting the German economy in the early 1990s.

A large body of literature has investigated the effects of immigration, but researchers are still far from reaching consensus: for example, a synthesis of wage effects for the US (see Dustmann et al., 2016) shows a range of values reaching from strongly negative (Altonji and Card, 1991; Borjas, 2003; Aydemir and Borjas, 2007; Borjas, 2014, 2017) to zero and even positive (Card, 2007; Card and Lewis, 2007; Card, 2009; Boustan et al., 2010; Peri and Yasenov, 2017).[3] The considerable variation in empirical results not only permeates US

[1] We define refugees as all displaced individuals in a third country who reside in a refugee camp, who have been formally given refugee status, or who have been granted temporary forms of protection (Dustman et al., 2016). For simplicity, we broadly consider as refugees all immigrants coming from a refugee sending country, though we acknowledge that this classification might be imprecise in some cases.

[2] Very recent examples include the rise of the AfD in Germany, FPÖ in Austria, Front National in France, and the PVV in Netherlands.

[3] See, e.g., Card and Peri (2016) for a summary of the ongoing dispute between George Borjas on the one hand, and David Card and Giovanni Peri on the other hand. Particular attention has been given to the mariel

studies, but also prevails in research focusing on European countries, and, most importantly for us, the German labour market. On the one hand, Bonin (2005), Haas et al. (2013), and Steinhardt (2011) report only modest (if any) wage effects, and Pischke and Velling (1997) find no detrimental effects on employment, though their analysis focuses on the 1985-1989 period, and thus misses the massive rise of inflows between 1990 and 1993.[4] On the other hand, Dustmann and Glitz (2015) [DG], considering a similar inflow as we do in this paper, and Prantl and Spitz-Oener (2014) who look at East German immigrants, discover significant wage losses in the nontradable and the competitive sector of the labour market. Meanwhile Velling (1995), looking at the 1989-1993 immigration shock, and also Glitz (2012), considering the 1996-2001 inflow of ethnic Germans, document a negative effect on employment rates. One study that simultaneously reports negative wage and employment effects, and is at the same time the most relevant for us, is a recent paper by Dustmann, Schönberg, and Stuhler (2017) [DSS]. Their preferred estimates imply moderate wage and large employment losses of about 0.13 and 0.93%, suggesting nearly perfect displacement of natives at the local level. While offering compelling identification, however, their analysis is based on a relatively small border region in south-east Germany.

While all these papers make important contributions, the large heterogeneity in results, even when based on similar data and time periods, makes it difficult to discern a consensus or compare results across studies. The problem starts with the definition of immigrants and natives. Roughly between 1985 and 1995, three groups of immigrants entered the German labour market: foreign migrants (that we consider below), ethnic Germans, and East Germans.[5] Since all three groups differed, *inter alia*, in their migration incentives, skills, and regional allocation, the empirical implications from studies considering these different groups are obviously not comparable. On top of that, some studies run into data issues identifying East and ethnic Germans as migrants (both held or received German citizenship upon arrival), raising concerns that any effect of immigration is driven by the allocation of these "German" inflows rather than changes in the resident native workforce. And even if studies use the same definition of immigrants and natives, they still rely on alternate measures of migration flows (skill-specific or overall), exploit different sources of variation (skill-cells, regions, or both), and include varying sets of control variables, implying that they identify conceptually different parameters that answer different questions (Dustmann et al., 2016).[6]

boatlift, first investigated in Card (1990). The original study finds no effects of marielitos on natives as does a recent analysis of Peri and Yasenov (2017) based on an improved identification strategy. This finding has been substantiated by Lewis (2004) and Bodvarsson et al. (2008) showing potential channels through which the immigrant shock might have been absorbed without affecting wages. In contrast, Borjas (2017) argues that marielitos did have a negative effect on wages, but only for high school dropouts.

[4] In addition, they use aggregate data for rather large regional units which, at a time when long distance commuting was more costly, might lead them to estimate a smaller employment response because cross-border job-to-job moves are not measured.

[5] Glitz (2012) analyses the inflow of ethnic Germans after 1995, exploiting a dispersal policy that allocated immigrants across districts. Prantl and Spitz-Oener (2014) investigate the inflow of East Germans using information on an individuals training occupation and residence in early childhood to identify a supply push to West Germany. D'Amuri et al. (2010) use the total inflow comprising of foreign immigrants, ethnic and East Germans.

[6] For example, many of the previous studies consider skill-specific migration flows (rather than overall flows),

In this paper, we provide the first comprehensive assessment of the refugee-driven immigration shock between 1988 and 1993 on the resident *native* workforce in the West German labour market. In contrast to some earlier studies (e.g. Bonin, 2005; Glitz, 2012; Dustmann and Glitz, 2015), we exclude East and ethnic Germans from our analysis, thus getting as close as possible to the impact of immigration on the resident native labour force. Our empirical design exploits spatial variation in immigrant inflows across commuting zones, enabling us to estimate the overall effect of immigration on various subgroups of the labour market, including skill and age groups, industries, and occupations. In doing so, we recast several wage and employment estimates from the literature in a general and consistent empirical framework. Unlike most existing empirical papers (also those based on longitudinal micro-data), we additionally trace out the dynamic effects of immigration, evaluate short run (5 years) and long run (10 years) effects, control for worker selection into employment, and evaluate the underlying adjustment mechanisms in our employment analysis. As indicated above, the inflow that we consider was of exceptional magnitude and dominated by refugees. However, while all refugee shocks share particular similarities, they also exhibit important differences that prevent direct conclusions about other refugee shocks such as today's. Most importantly, the refugee wave between 1988 and 1993 was presumably more substitutable to resident German workers as it encompassed many East European migrants who were often well educated and had basic knowledge of the German language. Despite this, we believe that a thorough understanding of the labour market effects of this particular immigration shock, and especially its dynamics, may help policy makers today to design more effective regulations in order to harvest the benefits of immigration.

Any study of the impact of immigration must deal with potential endogeneity of immigrant inflows. Since the immigration shock that we consider was largely composed of refugees — Yugoslavs fleeing the Balkan conflicts, Turks escaping violence against the Kurdish population, migrants leaving transition economies in the former Eastern Bloc states, and Kazakhs, Afghans, Iranians, and Lebanese driven out of their home country by ongoing conflicts — it is reasonable to assume that the timing and skill composition of the overall shock was largely exogenous.[7] Not so, however, the choice of the particular destination country, and, even more precarious for our case, the selection into a particular region within the destination country. For example, if these refugees, conditional on entering Germany, selected themselves into thriving regions with favourable employment prospects, simple correlations between local immigrant inflows and native wages or employment would be biased upward. To address this concern, many papers have exploited historical immigrant allocations to predict local immigration shocks that are uncorrelated with current demand factors (Card, 2001; Glitz, 2012; Peri and Sparber, 2009; Dustmann and Glitz, 2015). However, in our setting, we find such an instrument to lack reasonable power of

and therefore identify the distributional effects of immigration on native outcomes. In contrast, the political and public debate is often more broadly concerned with the overall migration flows. In this paper, we focus on the latter.

[7]As noted in Borjas and Monras (2016), refugee-driven supply shocks are plausibly exogenous along various relevant dimensions such as skills, magnitude, and the economic condition in the destination country.

predicting the pattern of current inflows, and therefore devise an instrument exploiting a region's distance to the south and east German border. This instrument rests on two particular features of the German migration history: first, the guest worker period between 1955 and 1968 which generated a substantial south-north gradient in immigrant employment shares and led immigrants two decades later to settle where earlier immigrants already resided;[8] second, the total blockade of East-West migration since 1961, which induced exceptionally low shares of foreign workers from the Eastern Bloc states, and led later immigrants to trickle into the country from east to west, often staying close to their home country for commuting reasons. Taken together, by exploiting a refugee-driven immigrant shock, by estimating the overall impact on various important subgroups of the labour market, and by analysing the dynamic effects over time, we are able to provide a comprehensive and consistent assessment of the impact of immigration on the resident native workforce for a leading European economy.

We begin our empirical analysis by estimating simple models relating the short run change in local native wages and employment to the associated overall inflow of immigrant employment between 1988 and 1993, instrumented using distance to border. Overall, our main result indicates that an inflow of foreign migrants has a negative short run effect on average native wages and employment: within five years, a one percentage point rise in local immigrant employment — around half of the average foreign employment inflow over that time period — reduces local wages and employment by about 0.68% and 1.13%, respectively. Taken at face value, the employment effect suggests an almost perfect short run displacement of native workers at the local level, where for every additional immigrant finding a job in a region, one native leaves or no longer enters employment in that region. These baseline results are robust to a variety of specification checks concerning the unit of observation, the choice of regions, the inclusion of further covariates, and alternative measures of the immigration shock and native outcomes.

It is important to bear in mind that our empirical estimates refer to a relatively short time frame (1988-1993), whereas many analyses of immigration consider decadal changes. In the short run, we expect negative responses to be more severe than in the longer term when firms might adjust their capital or production technology (DG; Lewis, 2011), workers might continue to settle in other regions or specialise in higher skilled occupations (Peri and Sparber, 2009), and future entrants might invest into more education (Hunt, 2017). In line with this reasoning, we find positive wage and employment effects in the post-shock period, 1993-1998, that are sufficiently large to compensate for the entire wage and employment reduction in the 1988-1993 period.[9] In addition, we find that local employment reductions

[8] We acknowledge that the initial allocation of immigrants that we exploit is driven by demand considerations, since guest workers moved to (or were assigned to) areas requiring additional labour at the time. To the extent that this implies a positive correlation with wage and employment developments during our analysis period, this would generate an upward bias in our estimates and lead us to understate the negative effect. Our results do not point to such effects, and we provide evidence suggesting that our instrument is indeed exogenous.

[9] This dynamic adjustment process is consistent with a recent study by Ruist, Stuhler, and Jaeger (2017), who document that an overlap of (negative) short run and (positive) long run responses to immigration shocks might explain why immigration studies reach different conclusions regarding the wage impact of immigrants.

might be associated with native job-to-job mobility between regions, and we show that on average roughly two-thirds of the local employment response can be traced back to such cross-regional moves. Importantly, this implies that the economy-wide employment decline is substantially smaller than suggested by our local estimates, even in the short run.[10]

To provide a coherent picture of the native response to immigration and to better understand the sources of these effects, we then assess the impact of immigration on natives with different levels of education and age, and in different types of occupations and industries. We find that employment losses are more important among unskilled than skilled workers, and stronger for workers above 30 than for labour market entrants, while wage reductions are, somewhat surprisingly, more pronounced among skilled and middle-aged workers. Looking across occupations and industries, we find that wage effects are concentrated among workers in simple occupations and in nontradable sectors, whereas employment effects are concentrated in tradable sectors, consistent with earlier evidence reported in DG.

Finally, we decompose the employment effect into inflows and outflows, distinguishing between cross-regional employment moves (job-to-job) on the one hand, and transitions between employment and nonemployment on the other hand. Overall, we find that both inflows and outflows contribute to the net employment decline, with around two-thirds attributable to declining inflows, and one-third to rising outflows. If we further differentiate between job-to-job moves across regional borders, and employment-nonemployment transitions, our key finding is that on average about two-thirds of the reduction in inflows sources in job-to-job transitions from other regions. This suggests that, as in DSS, firms react to an immigrant-induced labour supply shock by adjusting their hiring behaviour, insulating incumbent workers from negative effects of immigration. While geographic flows are an important determinant of inflows, we find that outflows are primarily affected by nonemployment flows. In particular, we see a significant rise in these nonemployment flows among workers above 50, consistent with strong incentives prevailing for this group of workers to retire early.

In the next two sections, we provide some background on the German history of immigration and introduce the data. We then describe our empirical design in section 3.4, and discuss our identification strategy in section 3.5. Section 3.6 presents our results, and section 3.7 concludes our analysis.

Their argument focuses on the shift share type of instrument, which is also central to our identification. Note, however, that in our setting, an overlapping response bias is unlikely to be an important concern as the two decades preceding our analysis period have seen comparably small fluctuations in immigrant inflows.

[10] To the extent that immigrants induce natives to switch jobs, the newly created job matches could potentially benefit natives. For example, underneath Peri and Sparber (2009)'s idea of native specialisation in communication-intensive or complex tasks is the notion of natives moving from one job to another.

3.2 Background and Macro-Trends

Immigration to Germany: To provide context, Figure 3.1 plots net foreign migration flows between 1972 and 2002, broken out by three major regions of origin.[11] The first group includes immigrants from guest worker countries including former Yugoslavia, Turkey, Greece, Italy, and Portugal; the second group consists of all major Eastern Bloc states such as Central and Eastern Europe, the former Soviet Union, Poland, and Romania; and the last group summarises all remaining inflows, primarily composed of Asian source countries, including Kazakhstan, Afghanistan, Iran, and Lebanon. While the figure reveals sizeable fluctuations in net migration throughout all years — caused by, e.g., the discontinuation of guest worker contracts in 1973, family reunification in the years to follow, and the economic recession of 1982 — the most striking observation emerges from the relatively short period between 1985 and 1995: within a decade, the stock of immigrants rose dramatically by 64% from 4.4 million to 7.2 million people, implying a net population growth of 5% relative to 1985.

The unprecedented surge of immigration between 1988 and 1993, in particular, was almost entirely composed of refugees from various flashpoints in Europe and Asia, among them, Yugoslavs fleeing the Balkan conflicts (27.2% of the total inflow), as well as Turks (21.9%), Kazakhs, Afghans, Iranians, and Lebanese, but also migrants leaving transition economies in the former Eastern Bloc states (Bauer et al., 2005).[12] This immigration shock (net inflows peaked at 600,000 immigrants in 1992) hit the German labour market and administration rather unexpected, and resulted in a gradual deterioration of the public opinion towards immigration which culminated in severe xenophobic riots and a profound reform of the asylum law. Indeed, the implementation of this law on July 1st, 1993, explains the substantial reduction of inflows from this year onwards (BAMF, 2004). In sum, the sudden rise and the abrupt reduction between 1988 and 1993 may be seen as a large-scale natural experiment (D'Amuri et al., 2010; Borjas and Monras, 2016) that allows to study the labour market effects on natives in an economic and public environment that was initially unprepared to deal with such large numbers of immigrant arrivals.

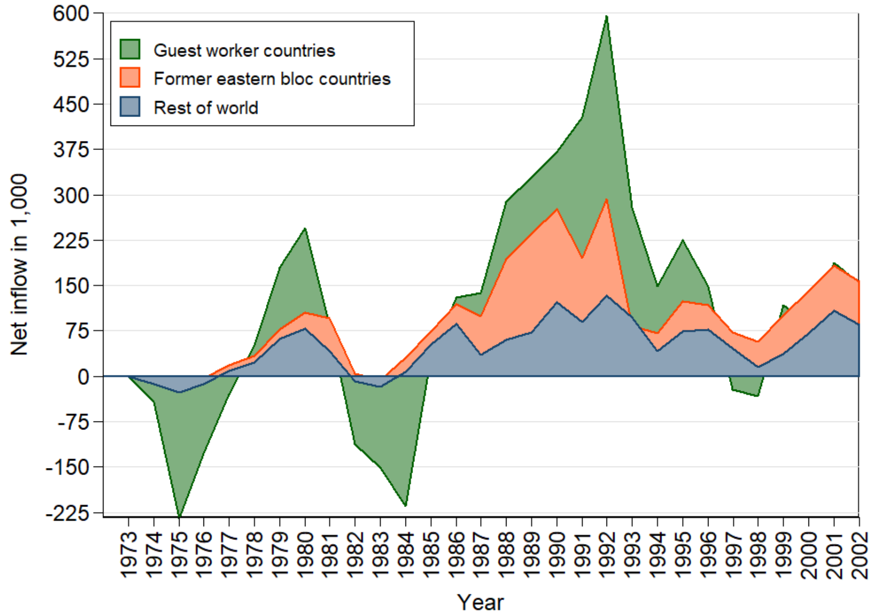
Immigrants and the Native Labour Market: An immediate concern arising from the previous discussion is whether these immigrants actually gained access to the labour market after arrival. Their refugee status, the lack of official certificates, delayed recognition of foreign degrees, or uncertainty about the permanence of stay generated severe impediments for refugee migrants to enter the job market. Also, legal working requirements were particularly restrictive until 1991, confining refugees' right to work throughout the asylum process (*Aufenthaltsgestattung*) and toleration status (*Duldung*).[13] As Figure 3.2 illustrates

[11] Data is available at the German Federal Statistical Office.

[12] For this reason, we will use the terms immigrant and refugee migrants in our analysis interchangeably. However, we note that this is a slight abuse of language as the total immigration flow also comprises of a rather small fraction of migrants from non-refugee sending countries.

[13] For background information on the regulatory framework, see Appendix B.2.

Figure 3.1: Total Immigrant Inflows by Region of Origin Between 1973 and 2002

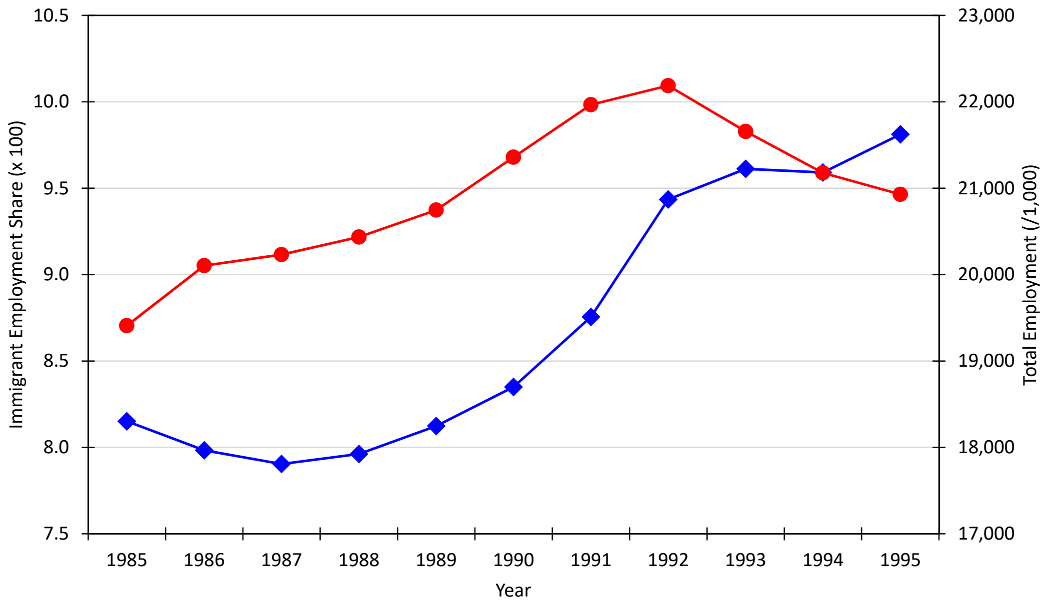


Note: Figure shows total net inflows (in thousands) from foreign countries to Germany between 1973 and 2002. We classify all nationalities into three major regions of origin (see main text). The figure illustrates the sharp rise of immigration between 1988 and 1993.
Source: Federal Statistical Office

based on our administrative data, the surge in overall population inflows indeed coincided with a parallel rise in immigrant employment rates by about 2 percentage points, equivalent to roughly 450 thousand additionally employed immigrants.[14] While less than the overall population growth (5%), this shock still constitutes a substantial immigrant-induced expansion of local labour supply, and suggests that we capture the bulk of inflows most likely to affect resident native labour. However, it is conceivable that some immigrants found a job outside the social security system, e.g., through the system of bilateral labour treaties (*Werkvertragsarbeitnehmerabkommen*) first established in 1988 with Czechoslovakia, Hungary, Poland, Yugoslavia, and Austria in an attempt to recruit workers for the building industry. According to official figures, these agreements sparked a rise in the number of temporary labour migrants from about 15,000 workers in 1988 to 95,000 in 1992 (Menz, 2009; Bundesamt für Migration und Flüchtlinge, 2006). We would suspect that these labour migrants provided less competition for natives than immigrants employed subject to social security, however, to test whether this "undercounting" of the employment shock still affects our results, we show in section 3.6.4 that our results are robust to excluding the building sector from our analysis.

[14]For example, between 1988 and 1993, total West German employment (subject to social security) rose from about 20.4 million to 21.7 million workers, suggesting that total immigrant employment increased from about 1.63 ($20.4 \times 8.0\%$) to 2.08 ($21.7 \times 9.6\%$) million workers.

Figure 3.2: Immigrant Employment Shares and Total Employment Counts Between 1985 and 1995



Note: Figure shows the immigrant employment shares calculated from our analysis sample (see main text) and the evolution of total dependent employment of natives for the West German labour market between 1985 and 1995. Total native employment is reported net off East and ethnic German employment.
Source: SIAB 7510

3.3 Data Set and Descriptive Overview

German Social Security Records: Our analysis is based on a representative 2% subsample of administrative records of all dependent employees subject to social security, provided by the IAB (*Institut für Arbeitsmarkt und Berufsforschung*). The data cover the years 1975-2010, and integrate information on employment as well as periods of registered unemployment.[15] Two reasons make this data set particularly useful for our analysis. First, it allows us to construct accurate measures of employment not only for natives but also for immigrants. While the number of immigrants entering the social security system between 1988 and 1993 underestimates the total number of new arrivals (recall that many immigrants were not allowed to work), we suspect that our data captures well that part of the inflow which is most relevant for the development of native wages and employment.[16] Second, our data allow us to track individuals across space, employment states, and over time, enabling us to

[15]The data are representative for about 80% of the German workforce. Excluded are self-employed, civil servants, full-time students, and the military; see vom Berge et al. (2013) for details.

[16]As commonly done in German data, we identify immigrants based on citizenship rather than country of birth (Bonin, 2005; D'Amuri et al., 2010; Glitz and Wissmann, 2017). We describe how we impute missing values in Appendix B.1. We also calculated the descriptive results below based on data from the German Socio-Economic Panel Study, which records the country of birth (rather than citizenship). We found similar trends, though as expected, the increase between 1988 and 1993 is somewhat more pronounced.

improve on many earlier studies by controlling for worker selection in wage regressions, and by casting light on the mechanisms underlying the employment effects.

Sample Restrictions and Variables: We restrict our analysis to all regular employed and unemployed male and female workers aged 18-64 in 204 geographically disjoint commuting zones covering the entire West German labour market, excluding Berlin (Koller and Schwengler, 2000).[17] From the resulting sample, which we refer to as "labour force", we draw two primary subsamples, one for our wage analysis and one for our employment analysis. For our wage analysis, we drop part-time workers since we only observe daily wages and the part-time status (no working hours), and include only workers observed in two consecutive periods in the same local labour market (see below).[18] In our employment analysis, we keep part-time employees, but weight them down by 1/2 or 2/3, depending on the particular part-time status (small vs. large). We distinguish between unskilled and skilled workers (based on the level of education) and between three age groups (18-29, 30-49, and 50-64). Individuals with at most a high school degree (*Abitur*) are considered unskilled, whereas individuals who completed an apprenticeship training or obtained a tertiary degree (e.g., Bachelor, PhD) are considered skilled.[19] While our main reason for choosing a two-skill classification is to avoid sample size issues in small local labour markets, this grouping also facilitates comparability of our results with Anglo-Saxon countries, where many occupations that require apprenticeship training in Germany demand a college degree.

Identifying East and Ethnic German Inflows: An important issue for our analysis concerns the definition of immigrants and the resident native labour force. As indicated earlier, our analysis period has not only witnessed a dramatic rise in refugee migration, but also experienced substantial inflows of East and ethnic Germans — both recorded as Germans in our data set.[20] These coincident inflows of “Germans” might confound our analysis for two main reasons: first, due to their German citizenship, they potentially enter our left hand side variable, thus generating an upward or downward bias in our wage and employment analysis, depending on their wage development and whether their allocation is positively or negatively correlated with the inflow of refugee migrants;[21] and second, these migrants constitute a shock to the resident native labour force in and of themselves, so their exclusion might result in an omitted variable bias. To address these concerns, we draw on selection rules to identify East and ethnic Germans in our West German analysis sample. Specifically,

[17] We exclude workers in training and in marginal employment because wages of trainees are unlikely to reflect an individual’s productivity and marginal employment is not consistently observed prior to 1999.

[18] Wages are top-coded at the social security contribution ceiling. We impute censored wages following the approach in Glitz (2012); see Appendix B.1 for details.

[19] Due to data limitations in the education variable, we impose some minor corrections; see Appendix B.1.

[20] According to statistics from the Federal Office of Administration (*Bundesverwaltungsamt*) and the Federal Statistical Office of Germany, about 1.65 million ethnic Germans and 1.45 million East Germans entered West Germany between 1988 and 1993.

[21] Note that, as explained below, our wage analysis is based on two-period regional stayers, suggesting that the influx of Germans with lower wage levels *per se* does not affect our estimates. However, if East and ethnic Germans featured smaller wage growth on average, our wage estimates would be downward biased.

following Glitz and Wissmann (2017) we define all individuals whose first employment spell indicates an East German location as East Germans, and exclude the complete employment biographies of these workers from our analysis. Moreover, we identify ethnic Germans by exploiting administrative information on the receipt of registered integration programmes such as language courses (Brücker and Jahn, 2011).

Local Labour Market Trends: Table 3.1 summarises our analysis sample for the years 1988 and 1993, calculated across employment weighted commuting zones. The next rows show that local immigrant shares rose by more than 30%, with the largest increases among unskilled (50%) and workers under age 30 (90%), but also sizeable gains among skilled and older workers. Comparisons of the age and skill structure between natives and immigrants show that the recent inflow of immigrants was disproportionately younger and lower educated than resident natives, offsetting the overall trend of ageing and skill upgrading in the population. Similarly, we find that the share of immigrants working in simple occupations (defined below) rises considerably over time, though, surprisingly, starting from a lower level than natives to begin with. The bottom rows show the evolution of average native and immigrant wages (in logs) as well as the difference between the two. During our analysis period, native wages rose by about 8.1 log points, compared to only 0.4 log points for immigrants, implying that the wage gap increased by 7.7 log points (1.54 log points p.a.). As illustrated in Table 3.2, using a series of fixed effects models, these basic patterns also hold within detailed education-experience groups, and within the same regions, occupations, and industries.

The standard deviation of immigrant shares noted in Table 3.1 points to large variations across regional labour markets. To draw a more comprehensive picture, Figure 3.3 plots the density of region level changes in immigrant employment shares between 1988 and 1993, weighting each region by total native employment in the base year. Overall, the distribution is roughly centred around the mean, and reveals a somewhat longer right tail, with some regions experiencing an increase in immigrant employment of up to 10%.

In Table 3.3, we list the 1988 and 1993 immigrant shares for the 30 *largest* commuting zones, ranked by their total labour force (natives+immigrants) in 1988. The table illustrates substantial variation in immigrant shares and inflows over time. For instance, within the 5-year period, the share of immigrants rises by 2.9 percentage points in Heidelberg, Nuremberg, and Aachen, and by only 0.4 percentage points in Braunschweig. Another interesting feature coming out of this table is the broader geographic distribution of immigrant shares: of the 30 regions listed in the table, 14 exhibit two-digit immigrant shares by 1993, and these are all located in south (8) or middle (6) Germany. Computing the average growth in the immigrant labour force share (col. 4) for north, middle, and south regions listed in the table yields values of 1.2, 1.8, and 2.5 percentage points, respectively. The dominating role of, especially, southern Germany with respect to the rise in immigrant employment goes back to the early settlements of guest workers in the 1950s and 1960s, and it constitutes a core element of our identification strategy.

Table 3.1: Summary Statistics of Local Labour Markets

	Year				Percent Change	
	1988		1993		1988-1993	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)	(5)	(6)
Immigrant Shares ($\times$100)						
Labour force share	8.0	(3.9)	9.8	(4.2)	32.1	(49.6)
Employment share	8.0	(3.9)	9.6	(4.2)	31.1	(65.6)
unskilled	23.1	(10.7)	30.5	(11.8)	50.4	(111.0)
skilled	4.5	(2.4)	5.6	(2.7)	36.8	(58.0)
age under 30	6.7	(3.3)	11.1	(4.9)	89.9	(106.8)
age between 30 and 49	9.6	(4.5)	10.0	(4.3)	12.8	(69.8)
age 50 and above	6.1	(3.6)	7.2	(3.6)	34.4	(65.7)
Age Distribution ($\times$100); above 50 omitted						
Share of natives below 30	30.5	(3.7)	25.8	(3.0)	-15.3	(4.9)
Share of immigrants below 30	25.1	(6.7)	30.1	(5.7)	26.1	(42.2)
Share of natives b/w 30 and 49	47.6	(2.6)	50.8	(2.2)	6.8	(5.3)
Share of immigrants b/w 30 and 49	59.1	(6.6)	53.1	(5.2)	-9.5	(12.5)
Skill Distribution ($\times$100); skilled omitted						
Unskilled share of natives	15.3	(15.3)	12.0	(2.4)	-21.5	(5.7)
Unskilled share of immigrants	53.8	(10.0)	50.9	(8.0)	-3.6	(19.6)
Occupational Distribution ($\times$100)						
Share of natives in simple occupations	63.8	(4.8)	65.5	(4.5)	2.6	(2.4)
Share of immigrants in simple occupations	50.8	(10.4)	57.6	(9.8)	16.0	(26.0)
Employment/Labour force rate ($\times$100)						
Average employment rate of natives	89.4	(2.7)	88.0	(2.0)	-1.5	(1.6)
Average employment rate of immigrants	88.6	(4.7)	85.7	(4.7)	-3.2	(6.1)
Wages (in 1995 Euro)						
Log average wage of natives (imputed)	4.33	(0.1)	4.41	(0.1)	8.1	(2.1)
Log average wage of immigrants (imputed)	4.21	(0.1)	4.22	(0.1)	0.4	(6.4)
Native-Immigrant wage gap	0.12	(0.1)	0.20	(0.1)	7.7	(6.2)

Note: Table shows summary statistics of the baseline analysis sample, calculated across local labour markets (commuting zones), weighting each observation by total native employment. The sample is restricted to West Germany and excludes ethnic and East German migrants (see main text and Appendix for details). Wages are deflated to 1995.
Source: SIAB 7510

3.4 Empirical Framework

3.4.1 Set-up

In our main analysis, we estimate models of the change in wages and employment of natives in group i and region r on the total region-specific immigrant inflow between 1988 and 1993.[22] Formally:

$$\Delta \log w_{irt} = \theta_{ir} T_r^{85-88} + \beta_i \Delta I_r^{88-93} + e_{ir} \tag{3.1}$$

and

$$\Delta N_{irt} = \psi_{ir} T_r^{85-88} + \gamma_i \Delta I_r^{88-93} + u_{ir} \tag{3.2}$$

[22]See DSS for a theoretical underpinning.

Table 3.2: Fixed-Effects Regressions (Worker Level)

	Native-Immigrant Wage Gap 1985 (1)	Native-Immigrant Wage Gap 1995 (2)	Annualised change (3)
Baseline	0.122	0.193	0.70
	(0.002)	(0.002)	
Baseline + region-fixed effects	0.155	0.226	0.71
	(0.002)	(0.002)	
Baseline + region- and occupation-fixed effects	0.048	0.095	0.47
	(0.002)	(0.002)	
Baseline + region- and occupation-industry fixed effects	0.044	0.079	0.35
	(0.002)	(0.002)	

Note: Table shows coefficients on a dummy for German nationality from regressions of the log wage (imputed) on a gender dummy and a quadratic polynomial of experience (baseline) plus fixed effects as indicated in the row heading. We fit the models on cross-sections indicated in the column heading. Column 3 calculates the annual percentage increase by dividing the difference of columns 1 and 2 by 10.
Source: SIAB 7510

where

$$\Delta I_r^{88-93} = \begin{cases} 0, & \text{if } t \in \{86, 87, 88\} \\ \frac{I_{r,93} - I_{r,88}}{N_{r,88} + I_{r,88}}, & \text{if } t \in \{89, \ldots, 93\} \end{cases}$$

and

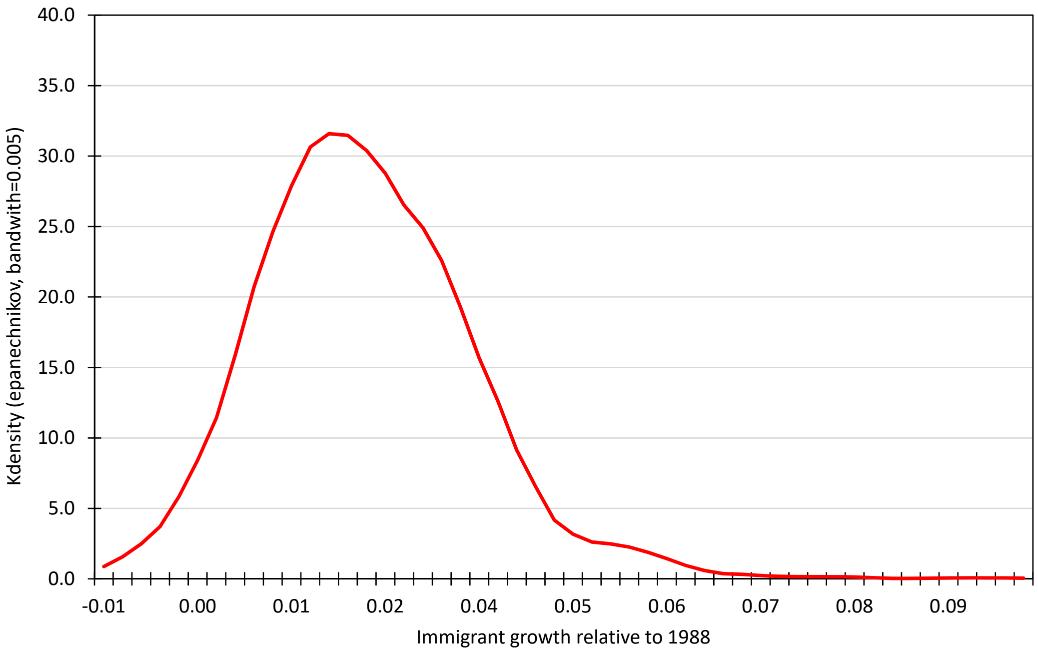
$$\Delta N_{irt} = \frac{N_{ir,t} - N_{ir,t-1}}{N_{ir,t-1}} \text{ and } \Delta \log w_{irt} = \log w_{ir,t} - \log w_{ir,t-1} \text{ for t} \in \{86 \ldots 93\}$$

We implement these models in first differences (losing the first year) and separately for each group i. Hence, in our baseline specification we eliminate region- and group-specific fixed effects, and allow the response of natives to differ across groups. We additionally take out differential wage and employment trends for subgroup i in region r by including linear region-specific time trends for the pre-shock period, $\theta_{ir}T_r$ and $\psi_{ir}T_r$, where $T_r = 1$ for years 1986-1988.[23] To identify the region-specific trend separately from the immigration shock, we accordingly set the immigrant inflow in these years equal to zero — in line with our discussion in section 3.2.

We estimate equations (3.1) and (3.2) using a two-step instrumental variable procedure: in a first step, we regress the log wage change or an indicator for a particular employment transition in each year on a full set of age and education indicators interacted with a gender dummy; we then aggregate the residual of these regressions at the region-year level and regress region-level first differences on the immigrant inflow, instrumented using distance to border. Our models therefore identify the impact of immigrants conditional on the

[23] The coefficients θ_{ir} and ψ_{ir} can be interpreted as the average annual wage and employment growth for subgroup i in region r between 1986 and 1988. We arrive at equivalent parameter estimates when subtracting the corresponding average from our outcome variable and running regressions on the years 1989-1993. Standard errors in this case are somewhat smaller.

Figure 3.3: Kernel Density Estimation of Changes in Immigrant Employment Shares Between 1988 and 1993



Note: Figure shows kernel estimates of region level changes in immigrant employment shares between 1988 and 1993. Estimation is weighted by a region's total native employment in the base year, and uses an Epanechnikov kernel with bandwidth 0.005.
Source: SIAB 7510

demographic structure in a regional labour market.

Our main interest concerns the parameters measuring the average subgroup-specific impact of an inflow of immigrants on native wages and employment in a region: β_i and γ_i. In our setting, these coefficients can be interpreted as the percent change in wages and employment of subgroup i in response to a one percentage point change in local immigrant employment between 1988 and 1993. They incorporate complementarities between skill and age groups as well as between capital and labour, and they accommodate the possibility of heterogeneous labour supply elasticities (or wage rigidities) for different demographic groups. Note that our immigration shock refers to a region as a whole — that is, we investigate how different native groups within a regional labour market are affected by an overall inflow of immigrants, irrespective of the specific composition of that inflow and the implied relative immigration shock for different native groups. We believe that this is a more policy relevant analysis, primarily because the demographic structure of immigrant inflows is often hard to measure (especially at the regional level), and devising appropriate regulations based on the particular composition of immigrants, while potentially superior in terms of effectiveness, seems difficult to achieve in practice.[24]

[24]One reason is downgrading of immigrants, which refers to a systematic difference between the position of an immigrant in the labour market (e.g. measured by the position in the wage distribution) and the position of a

Table 3.3: Distribution of Immigrant Labour Force Across Local Labour Markets

ID	Name of Region *usually largest single region*	Total LF in 1975 (1)	Immigrant Share 1988 (2)	Immigrant Share 1993 (3)	Difference 1988-1993 (4)
8	Hamburg	930,000	7.5	9.1	1.6
120	Stuttgart	929,450	16.4	18.5	2.1
159	München	914,150	13.9	16.8	2.8
92	Frankfurt/Main	852,150	14.0	16.4	2.4
45	Düsseldorf	606,650	10.6	12.3	1.7
57	Köln	587,900	11.6	13.5	1.9
17	Hannover	412,000	7.1	8.1	0.9
46	Duisburg	389,550	7.9	9.6	1.7
73	Dortmund	361,250	6.7	7.9	1.2
185	Nürnberg	359,650	9.1	12.0	2.9
63	Gelsenkirchen	335,400	7.5	9.0	1.4
47	Essen	278,750	6.5	8.0	1.5
42	Bremen	266,600	4.8	5.9	1.0
118	Saarbrücken	250,350	6.1	8.6	2.5
128	Karlsruhe	240,800	10.2	12.9	2.8
130	Mannheim	223,850	9.3	11.7	2.4
52	Wuppertal	220,950	10.9	12.8	1.9
67	Bielefeld	212,600	7.7	9.4	1.7
59	Bonn	208,450	8.3	9.7	1.5
6	Kiel	200,050	2.9	3.7	0.8
114	Ludwigshafen	188,300	7.9	9.2	1.3
64	Münster	187,500	4.1	5.8	1.6
200	Augsburg	185,100	9.5	11.7	2.1
129	Heidelberg	183,450	9.5	12.4	2.9
56	Aachen	163,650	9.0	11.9	2.9
9	Braunschweig	162,550	4.1	4.4	0.4
75	Lüdenscheid	153,300	11.1	12.4	1.3
72	Bochum	147,600	5.8	7.5	1.7
135	Freiburg	146,750	7.6	10.0	2.4
81	Kassel	146,600	6.2	7.1	0.9

Note: Table shows the 30 largest local labour market regions in 1988. The labour force is calculated as the sum of employed and unemployed individuals and multiplied by 50. Columns 2-3 show the share of immigrants in the total labour force calculated based on the labour force data for the year indicated in the column heading. Entries in column 4 show the percentage point change between 1988 and 1993.
Source: SIAB 7510

Following DSS, we measure the immigrant supply shock using the change in the number of *employed* immigrants as a fraction of total (native + immigrant) employment in the base period. This approach bears two issues relevant for the interpretation of our estimates. First, by relating the inflow to a fixed measure of employment in the base year, we avoid confounding changes in immigrant inflows with potentially correlated changes in native employment. As shown formally in Card and Peri (2016), the alternative specification based on changes in the share of employed immigrants might generate a downward bias in wage estimates, and is mechanically negatively correlated with changes in native employment

native with the same observed education/experience level. Based on a simple imputation procedure described in Dustmann et al. (2016), we find substantial downgrading in our analysis period: the effective share of unskilled immigrants (who entered the German labour market within the last five years) is on average 25% higher than the share of observed unskilled immigrants.

as dependent variable.[25] Second, by focusing on employment inflows divided by total employment in the base period, a coefficient of -1 implies that for each additional immigrant employed, one worker who is already in the country leaves or no longer enters employment in a region. This worker is native (foreign) with probability equal to the share of natives (foreigners) in total employment — we summarise these probabilities in the top panel of Table 3.1. By focusing on the subgroup of immigrants who find a job, however, we might miss indirect effects on natives from immigrants who also arrived but did not find a job.[26] To see how the definition of the immigrant shock affects our results, we also used labour force and population measures to gauge the immigrant inflow. However, these alternative measures led to very similar conclusions, both qualitatively and quantitatively.

3.4.2 Implementation Issues

Controlling for Worker Selection: Since our data are longitudinal in nature, we can account for selection into employment in wage regressions even though our unit of analysis is the region. In particular, before performing the covariate-adjustment at the individual level (see above), we focus on only those workers who are employed full-time in the same region in two consecutive years.[27] In so doing, we eliminate the change in wages between two periods that is generated by compositional adjustments in local employment (which might be caused by immigrants in the first place). If low wage workers are more strongly affected by immigrants than high wage workers — e.g., because immigrants are overrepresented in the low wage service sector —, and if their labour supply elasticity is relatively large — e.g., because they are more often employed under temporary contracts — not controlling for worker selection would lead to upward biased wage effects.

However, although restricting attention to stayers eliminates a composition bias in terms of wage *levels*, this approach may come at the cost of selection on wage *changes*. For example, if stayers are not only high wage but also high wage *growth* workers, our estimate of the impact of immigrants would be underestimated. We believe that this is less of an issue for two main reasons: first, comparisons of the variance of wage levels and wage changes show that the former is much larger than the latter. So, by eliminating the potential bias in wage levels, we likely account for the quantitatively more important bias. Second, we selected all (geographic) movers and stayers during the treatment years (1988-1993), and plotted their wage growth eight years earlier. Although the sample of movers is much smaller than the sample of stayers, we find remarkably similar wage growth distributions across all years.

[25] The key issue here is that changes in shares are downward biased if natives move into similar regions as do immigrants, e.g., due to positive demand shocks attracting both natives and immigrants, and implying higher wage and employment growth. Studies using this measure often draw a more negative picture of immigration shocks on native labour markets (Borjas, 2003, 2014; Bonin, 2005; Steinhardt, 2011) than papers using a specification that avoids this bias (Card, 2001, 2007, 2009; Peri and Sparber, 2009).

[26] One possibility is that employers, faced with a larger labour supply, are able to use this as a threat to enforce lower wages for the extant workforce. One could also think of the reverse effect, where native employees increase their effort in the wake of immigrants waiting to 'take their jobs'.

[27] This procedure is similar to first-difference regressions estimated at the worker-level, with differences taken within worker-region spells.

Cumulative Coefficients: In our empirical implementation of equations (3.1) and (3.2), we decompose the overall impact of the 1988-1993 immigrant shock on natives, β_i, into a series of annual effects, β_{it}. We then calculate the cumulative effect over a particular (flexible) time period, say, 1988-1993, by adding up the parameters from the annual regressions, i.e., $\beta_i = \sum_{t=1989}^{1993} \beta_{it}$. The β_{it}'s are interesting, as they help us to understand how local economies adjust to immigration shocks: technological adjustments (Lewis, 2011; Peri, 2012; DG), mobility of labour (Card and DiNardo, 2000) and capital, occupational specialisation (Peri and Sparber, 2009), and also land and housing prices (Saiz, 2003, 2007) are just a few of many potential margins of response. What they all have in common, though, is that their adjustment takes years to fully unfold, and our analysis of the dynamic adjustments over about a decade might provide evidence in favour of these mechanisms.

Two-Step Implementation of IV-Approach: Unlike conventional two-stage least squares (2SLS), we implement the first stage of our IV-models separately from the second stage as this enables us to adopt a different weighting scheme in each step of the estimation. In our first stage models, we want to weight by total native employment in 1988, first, because our immigrant shock refers to the region as a whole and is scaled to 1988, and second, because it accommodates the analysis of heterogeneous effects in response to the *same* shock. In our second stage models, however, the dependent variable refers to the subgroup-region-year, and hence, the appropriate weight should also refer to this cell. To account for the omitted first stage uncertainty in our "plug-in OLS" approach, we compute bootstrapped standard errors using a pairs bootstrap and 1,000 replications.[28]

3.5 Instrumental Variables

Immigrants are more likely to settle in regions that experience positive demand shocks over time, generating an upward bias in OLS estimates of both $\hat{\beta}_i$ and $\hat{\gamma}_i$. Since we estimate models of eq. (3.1) and (3.2) in first differences, we eliminate time constant region- and subgroup-specific heterogeneity in wage and employment levels that might be correlated with immigrant inflows. While region-fixed effects take out many structural differences that are correlated with wage/employment levels (such as industry and occupation structure), subgroup-specific effects eliminate heterogeneity between age and skill groups. In addition, subgroup-region specific time trends control for differences in wage and employment *growth* prior to the immigrant shock. Still, there is space for omitted variable bias if shocks within regions and subgroups simultaneously affect immigrant inflows and native outcomes. To account for this possibility, we devise an instrumental variable strategy based on the (airline)

[28] Another way of accounting for the first stage uncertainty would be to interpret the first stage as pre-estimation step in which we generate the instrument, and then implement a regular two-stage least squares regression using the generated immigrant inflow as instrument (Wooldridge, 2010, ch. 6.1.2). We show in Table B.1 that both the "Generated IV" and the "plug-in OLS" approach yield similar results.

distance between a local labour market and the southern (1) and eastern (2) German border.[29] This choice is guided by the specific composition of inflows indicated in section 3.2. We explain the details and summarise our results in the following.

Distance to Southern Border: The predictive power of distance to the southern German border dates back to the guest worker period during the 1950s/60s. Shortly after World War II, the German economy faced a shortage of labour caused by the preceding war period and draining inflows from East Germany. To fuel industrial production at home, politicians negotiated multiple recruitment agreements (*Anwerbeabkommen*) with several South European states, who themselves were facing high structural unemployment rates at the time. Hundreds of thousands of immigrants moved to Germany in the following years, crossing the southern border, and starting to settle in southern Germany (primarily Bavaria and Baden-Wurttemberg) before starting to move north (North Rhine-Westphalia). These allocation choices, which were primarily demand-driven due to industrial melting pots in these areas, ultimately generated a south-north gradient in immigrant shares. While their settlement was meant to be transitory, many immigrants stayed after the last recruitment halt in 1973, beginning to reunify their families and thereby consolidating the temporary immigrant enclaves.[30] Since new immigrants tend to settle where earlier immigrants had moved to before, our distance-to-south instrument predicts the allocation of the large fraction of immigrants coming from former guest worker countries between 1988 and 1993.

Distance to Eastern Border: To target the settlement of another large group of immigrants between 1988 and 1993, those coming from former Eastern Bloc states (excluding ethnic and East Germans), we use distance from eastern border as a second instrument. Prior to the fall of the inner German border, immigrants coming from the former Eastern Bloc accounted for only 0.3% of the total German population and about 3-4% of all immigrants. Following the collapse of the Soviet Union, however, immigrants from these regions came in large numbers (see section 3.2), and they began to trickle into the Federal Republic from east to west. Their settlement decision was not affected by prior compatriot settlements (as those were negligible), but instead reflected a trade-off between employment prospects on the one hand (inciting them to move to West Germany) and proximity to their home country on the other hand (inciting them not to move too far west). Travel costs were important for a large fraction of immigrants who worked during the week and returned to their families at weekends or migrated temporarily for several weeks or months (Moritz, 2011). Based on these considerations, distance from today's eastern border is another useful predictor of immigrant settlements between 1988 and 1993.

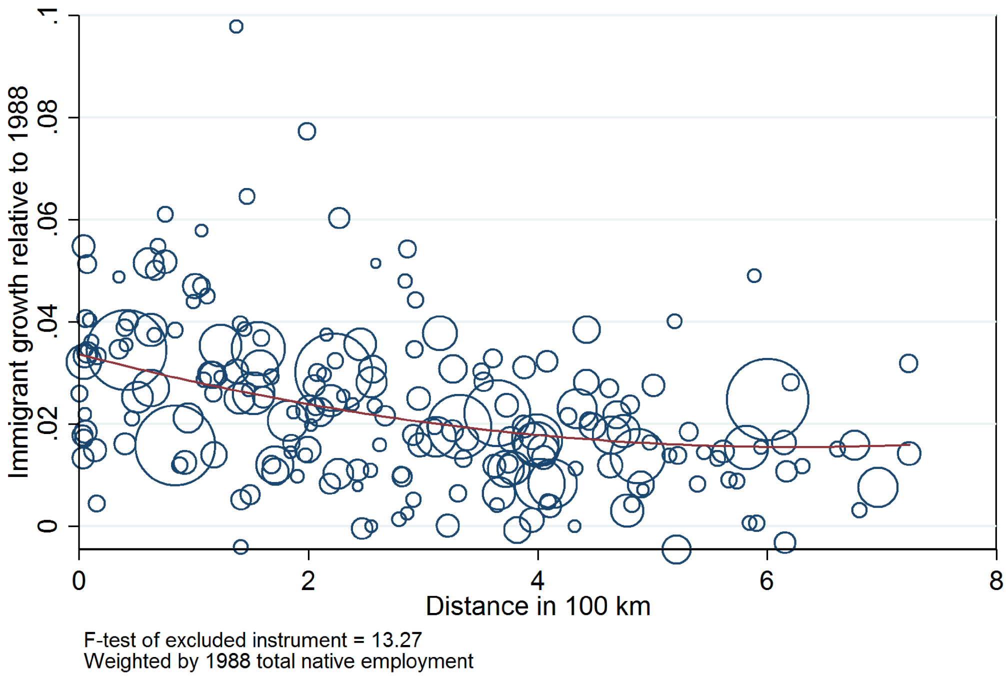
First Stage Results: Figure 3.4 illustrates the association between changes in local immigrant employment shares in 1988-1993 and the airline distance (in 100 km) from the

[29]Distance instruments have been widely used in the immigration literature, most often in the US context; see, e.g., Peri (2012); Smith (2012); Peri and Sparber (2009); Llull (2017).

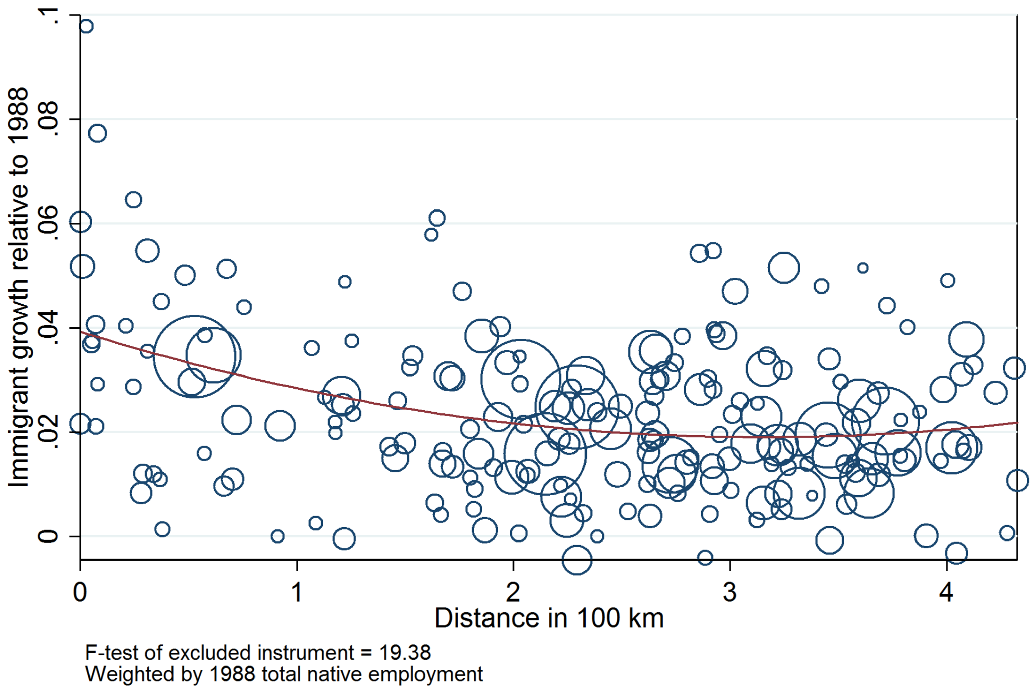
[30]Already in 1965 the author Max Frisch epitomised the situation, saying: "We called for workers and people came".

Figure 3.4: Spatial Distribution of Immigrant Inflows in West Germany Between 1988 and 1993

A. Distance to South German Border



B. Distance to East German Border



Note: Figure plots immigrant employment growth rates for all 204 commuting zones between 1988 and 1993 against distance to the border. Panel A shows distance to the southern border and panel B to the eastern border. The area of each circle is proportional to a regions employment share in 1988.
Source: SIAB 7510

Table 3.4: Summary of First Stage Regressions

	Foreign Immigrants					Ethnic Germans	East Germans	
	Distance to South and East Border	Distance to South	Distance to East	Average Distance	Shift share IV using '75 density	Distance to South and East Border	Distance to East Border [SIAB]	Distance to East Border [MZ]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance to South	-0.050	-0.058				0.032		
	(0.018)	(0.017)				(0.016)		
Distance to South sq.	0.004	0.005				-0.005		
	(0.003)	(0.003)				(0.002)		
Distance to East	-0.103		-0.128			0.074	0.011	-0.004
	(0.036)		(0.032)			(0.029)	(0.008)	(0.001)
Distance to East sq.	0.020		0.020			-0.014	-0.004	0.001
	(0.008)		(0.007)			(0.006)	(0.002)	(0.000)
Average Distance				-0.073				
				0.016				
Average Distance sq.				0.005				
				0.002				
Predicted %-growth 1988-1993					0.045			
					(0.025)			
R-squared (adjusted)	0.250	0.201	0.155	0.224	0.027	0.096	0.070	0.319
F-statistic (excl. Instr.)	16.43	13.27	19.38	30.47	3.34	2.90	12.26	13.82
Hansen's J (p-value)	0.707	0.475	0.250	0.423		0.628	0.898	0.105
Local labour markets	204	204	204	204	204	204	204	70

Note: Table summarises the first stage results. Columns 1-4 report the coefficients from models of the 1988-1993 change in immigrant employment on distance to border (divided by 1,000 km). Column 5 shows an alternative instrument using the 1975 distribution of immigrants by country of origin across local labour markets, interacted with the aggregate cumulative inflow from the same region of origin between 1988 and 1993. Column 6 uses the change in ethnic German employment instead of the change in foreign immigrant employment as dependent variable, and in columns 7 and 8, we use the change in East German employment. In column 7, we use data from the SIAB-sample, and in column 8, we use data from the German Microcensus as dependent variable. We use published data from Burchardi and Hassan (2013) to implement these regressions, and refer to their paper and Online Appendix for details.
Source: SIAB 7510; German Microcensus 1991, 1993, 1995

southern (panel A) and eastern (panel B) border. Each circle represents a local labour market, with its area being proportional to native employment in 1988. In line with our previous considerations, we find that regions closer to the border experienced higher immigrant inflows than regions further away. For example, the airline distance between Stuttgart and Hamburg, about 530 km, suggests that the change in immigrant employment between 1988 and 1993 was about 1.7 percentage points lower in Hamburg than in Stuttgart. A more detailed summary of our first stage relationships is provided in columns 1 to 4 of Table 3.4.[31] Our preferred specification, reported in column 1, includes distance to south and east separately. It explains about 25% of the cross-regional variation in immigrant growth rates (adjusted R^2), and the coefficients are jointly significant (F-statistic=16.4). As shown by Hansen's overidentification test, we cannot reject the joint validity of our instruments (p-value=0.707). In the next two columns, we examine each distance instrument separately. As suggested by Figure 3.4, the negative relationship is rather strong in both cases, generating

F-statistics of 13.3 and 19.4. In column 4, we combine the two distance measures into a single *average* distance from border. Compared to our preferred model, this yields a higher F-statistic (30.47), but a smaller adjusted R^2 due to the relative loss of information.[32]

Our distance-to-south instrument exploits a similar source of variation as the standard shift share instrument (though dating back to the 1950s/60s, and thus earlier than available data for regional immigrant densities), and it is natural to wonder how such a shift share instrument performs relative to our distance measure. We investigate this in column 5, finding only a weak association between predicted and actual immigrant employment growth (F-statistic=3.43), which disqualifies this instrument for our analysis.[33]

Confounding Effects of East and Ethnic Germans: Between 1988 and 1993, two other large waves of immigrants entered the West German labour market: East Germans and ethnic Germans. Although we exclude these migrants from the outcome variables (focusing solely on resident native workers), we might still be concerned with a potential omitted variable bias (see section 3.3). On the one hand, if East Germans located in the same regions as refugee migrants, and if East Germans also put downward pressure on native outcomes, then our coefficient estimates would be more negative. On the other hand, if East Germans avoided regions with high foreign inflows, perhaps because they expected better employment opportunities in unaffected regions, our estimates would be biased towards zero. To probe into this issue, we formed a new set of first stage models using proxies for the regional inflows of ethnic Germans (as identified above) and East German immigrants as dependent variables. To measure the inflow of East Germans, we use two approaches. First, we approximate the inflow of East Germans based on the SIAB sample, with the obvious shortcomings of being available only from 1992 onwards, and relying on a rather crude identification based on the location of the first spell (see above). Second, we draw on information from the German Microcensus, an annual survey of a 1% random sample of the German population. This survey asked individuals in 1991, 1993, and 1995, whether they had migrated from East Germany, and we thus use the change in the share of East German immigrants in West German regions as dependent variable.[34] We suspect that this measure is somewhat more reliable, though it suffers from the limitations of a smaller sample and potential measurement error. The relationship between these "German" inflows and our distance instrument is reported in columns 6-8 of Table 3.4. For ethnic Germans, we only find a relatively low correlation between inflows and distance to border, with an F-Statistic of less than 3. For

[31] Similar results emerge when we exclude the quadratic term or use a spline with 10 knots.

[32] We define the average distance as $\overline{d} = \left(d(south)^2 + d(east)^2\right)^{1/2}$, which yields the smallest values for regions in south-east Germany and is increasing in northern and western direction.

[33] In Appendix B.5, we provide a detailed description of the construction of our shift share instrument. Moreover, we compare our approach with Dustmann and Glitz (2015) who provide a similar application of the instrument for West Germany during the same time frame but obtain a highly significant relation between the predicted and the actual change in local labor supply.

[34] Specifically, we draw on data prepared by Burchardi and Hassan (2013), and refer to their study for further details of the sample construction. They use a coarser definition of regional units and compute the difference in the share of East Germans in West Germany, rather than the change of East Germans in the West German population. Moreover, they refer to the total population rather than the number of employed individuals.

Table 3.5: Baseline Effects of Immigration on Native Wage and Employment Growth

	Wages			Employment		
	'88-'93	'93-'98	'88-'98	'88-'93	'93-'98	'88-'98
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pooled sample						
OLS	-0.108	0.120	0.010	0.186	0.739	0.920
	(0.112)	(0.127)	(0.199)	(0.351)	(0.278)	(0.554)
2SLS	-0.677	0.527	-0.153	-1.125	1.504	0.377
	(0.281)	(0.187)	(0.393)	(0.718)	(0.474)	(1.096)
Panel B: Unskilled						
OLS	-0.089	-0.120	-0.212	-0.152	0.977	0.821
	(0.213)	(0.218)	(0.363)	(0.646)	(0.542)	(1.039)
2SLS	-0.695	-0.101	-0.725	-2.610	1.113	-1.513
	(0.459)	(0.321)	(0.607)	(1.166)	(0.789)	(1.575)
Panel C: Skilled						
OLS	-0.068	0.244	0.175	0.240	0.766	1.003
	(0.122)	(0.133)	(0.216)	(0.376)	(0.314)	(0.615)
2SLS	-0.581	0.843	0.245	-0.917	1.711	0.799
	(0.294)	(0.208)	(0.428)	(0.779)	(0.558)	(1.295)
Local labour markets	204	204	204	204	204	204

Note: Table shows cumulative wage (columns 1-3) and employment (columns 4-6) effects from a series of models relating native annual wage and employment changes to the aggregate inflow of immigrants between 1988 and 1993. Two-stage least squares estimations are implemented in two steps as described in the main text. The first stage is weighted by initial native employment in 1988, and the second stage is weighted by native employment in the base year. Estimates are trend-adjusted using a region-specific linear trend based on years 1986 to 1988. Standard errors are calculated using a pairs bootstrap with 1,000 replications.
Source: SIAB 7510

East Germans, in contrast, we find a relatively strong correlation arising in both the SIAB and the Microcensus.[35] As this finding might cast doubt on our main results, we show in section 3.6.4 that our key conclusions hold up when we additionally exclude regions with unusually high inflows of natives in the critical years (potentially East or ethnic Germans that are not correctly identified), or regions within an 80km strip from the former inner German border (section 3.6.4).

3.6 Results

3.6.1 Baseline Effects

Table 3.5 summarises our baseline results of the impact of immigrants on local native wages and employment based on eq. (3.1) and (3.2). Looking across the first row, reporting simple OLS effects, we first note that the growth in immigrant employment is uncorrelated with changes in native wages and employment. These simple correlations might be upward biased by positive demand shocks jointly attracting immigrants *and* generating higher wage and employment growth for natives. Instrumenting the change in immigrant employment with

[35]Moreover, informal experimentation with German pension data suggests that this correlation breaks down if we focus on employment inflows rather than population inflows.

distance to border, we find considerably more negative effects. In particular, our coefficient estimates suggest that a one percentage point rise in local immigrant employment — about half of the average increase between 1988 and 1993 — reduced native wages and employment by 0.68 and 1.13%, respectively, though the employment effect is somewhat imprecisely estimated. To put the wage effect into perspective, note that over the same time period native real wages rose by about 1.3% per year (8.5% over 5 years; see Table 3.1), suggesting that the negative impact of immigrants did not result in real wage losses for natives, but rather counteracted what would otherwise have been an even larger wage growth.

To understand how these cumulative effects evolved over time, Figure 3.5 plots the sum of the 2SLS effects in each year relative to 1988, which we normalise to zero.[36] Two main conclusions emerge: first, our estimates for both wages and employment are close to zero and statistically insignificant before 1988 when immigrant inflows were low, suggesting that distance to border is uncorrelated with native wage and employment growth prior to the inflow of immigrants. This finding alleviates concerns that our instrumental variable strategy merely picks up persistent labour demand differentials between regions, e.g., due to different industry compositions, and thus serves as an indirect confirmation of the validity of our identification strategy (DSS). Second, coinciding with the rapid surge of immigration from then on, we observe a steady downward trend in wages until 1993, followed by a relatively flat development thereafter. Reassuringly, this pattern is just the reciprocal of the rise in immigrant employment shares over the same years (Figure 3.2). While the overall patterns are similar for employment, the effect starts somewhat delayed, is significant only in 1992, and tends to be compensated in the medium run (after 1993) by positive annual effects. Taken at face value, the point estimate for 1992 suggests a sizeable displacement effect of about 1.02 employed natives per additional immigrant finding a job (-1.111×0.92).[37]

How can these estimates be reconciled with the positive or null effects often found in the literature? We argue that a key difference is *time*. In the short run, firms might be reluctant or unable to adjust their capital and production technology, workers might not have found a job in another region or upgraded to higher skilled occupations yet, and future entrants might still be investing into more education (see above). All these adjustment processes bear the potential to turn a negative short run effect of immigration into a null or positive effect in the medium or long run (Ruist et al., 2017). To shed more light on this, we investigate in columns 2-3 and 5-6 how wages and employment evolve over the 5-year period following the immigration shock (1993-1998), and over the entire 10-year time frame (1988-1998). Indeed, the overall picture is now considerably more optimistic, with significantly positive effects for both outcomes in 1993-1998 compensating for the negative effects in the short run, and implying a null effect in the long run. However, while these patterns are consistent with standard economic theory (Borjas, 2009) and empirical results in Monras (2015) and

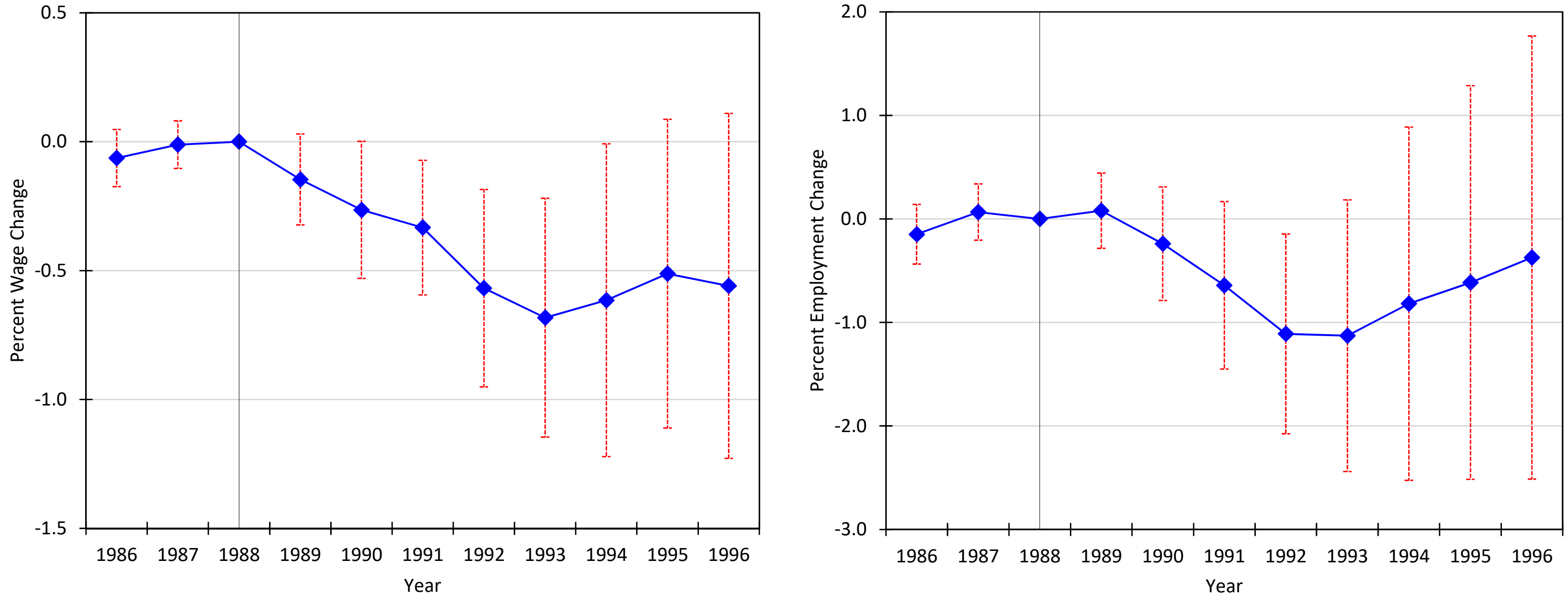
[36] This means that the difference between t and t+1 is equal to the coefficient estimate for t+1. For simplicity, we show confidence bands based on non-bootstrapped standard errors which are slightly smaller than bootstrapped standard errors.

[37] See Appendix B.3 for a derivation of the native displacement effect. DSS, who also focus on the short run, report estimates in a similar ballpark (0.9 natives per additional Czech immigrant employed).

Figure 3.5: Cumulative Wage and Employment Effects of the 1988-1993 Immigrant Inflow

A. Cumulative Wage Effects

B. Cumulative Employment Effects



Note: Figure shows cumulative wage and employment effects for years 1986-1996. In each year, we plot the sum of coefficient estimates relative to 1988, that is, we sum backward and forward. 95-percent confidence bands are indicated in red. The vertical line represents the start of the immigration shock.
Source: SIAB 7510

Edo (2017), they should be regarded suggestive since, as we expand the analysis period, potentially confounding shocks might debilitate a causal interpretation.[38]

As indicated, these dynamic adjustments are consistent with various potential explanations. However, we believe that the magnitude of the short run effects calls for further explanations, and we investigate several possibilities below. We show that employment losses in one region are to a large extent associated with employment gains in other regions, and demonstrate that employment declines are concentrated in particular subgroups, among them the elderly for whom an outflow from employment might be associated with early retirement, rather than job search. We also show that the net reduction in regional employment results from both a slowdown in inflows and an acceleration of outflows, meaning that, unlike the typical notion of native displacement, part of the local employment loss is attributable to immigrants preventing native workers from finding a job in a region when they otherwise would have.

3.6.2 Skilled and Unskilled

The discussion in section 3.3 (Table 3.1) showed that immigrants were disproportionately unskilled, hence it is natural to wonder how different skill groups responded to the immigration shock. Indeed, panels B and C indicate some striking differences: for unskilled workers, we find no wage effects, but substantial employment effects of about -2.61%, which, on a per worker basis means that about 0.37 unskilled natives leave or no longer enter employment for each additional immigrant employed between 1988 and 1993.[39] This suggests that unskilled workers bear the burden of immigration predominantly through the employment margin: although they only account for about 15% of local native employment, they account for 35% (0.37/1.04) of its decline. In contrast, our estimates for skilled workers suggest that they respond primarily through wage losses.[40] Taken together, these findings point to a relatively larger short run labour supply elasticity (or stronger wage rigidities) for unskilled than skilled workers, meaning that part of what would otherwise have been a wage loss is compensated by a reduction in employment. These results underpin that a thorough evaluation of immigration effects must consider both wages *and* employment jointly. Specifically, if we had only examined wages (*assuming* inelastic labour supply), we would have found only skilled workers to be affected negatively by immigration, and our

[38] A related concern of long run estimates in settings that use cross regional variation is a potential violation of the stable unit treatment value assumption (SUTVA). As we show below, we find substantial spillover effects induced by native regional mobility, meaning that as time passes, our counterfactual regions also experience an (indirect) impact of immigrants through native mobility. It is thus difficult to interpret the long run estimates as an overall causal effect.

[39] Using figures from Tables I (col. 1: native share = 0.975), II (col. 1: 0.276) and IV (col. 2: -1.371) from DSS, we also obtain a displacement of 0.37 unskilled per additional Czech immigrant employed. This comparison holds only approximately, because DSS report summary statistics including the unemployed (leading to a higher unskilled share), whereas our summary statistics refer to employed workers only, which should be used to calculate the native displacement effect.

[40] This corresponds to a native displacement of about 0.71 skilled workers. In principle, the sum of unskilled and skilled displaced workers should sum up to 1.04 (-1.125×0.92), the total effect in 1993. They do not add up exactly due to the different weighting used in our second stage analysis.

estimates suggest that this conclusion would have been severely misguided.[41]

As before, we also examined how the effects evolve over time (col. 2-3 and 5-6). With one exception (wages for unskilled), the post-migration period is marked by positive wage and employment effects that are highly significant among skilled workers, and tend to mitigate the short run contemporary impact of immigration.

3.6.3 Age Groups, Occupations, and Industries

We next narrow our attention to workers in different age groups (young, middle, and old), and in different types of occupations (simple and advanced) and industries (tradable and nontradable), seeking to provide a coherent picture of the impact on the native labour market, and to better understand the proximate sources of these effects. We will focus on the short run effects, not only because the existing immigration literature has paid much less attention to this time horizon, but also because the short run dynamics constitute an essential ingredient for understanding how local labour markets adjust to immigration shocks.

Young, Middle, and Old Workers: Panel A of Table 3.6 shows 2SLS estimates of the impact of immigrants on young (under 30), middle (between 30 and 49), and old (above 50) workers. Since younger workers are both more mobile and on a steeper gradient of their age-earnings profile (making it easier for employers to enforce reduced wage growth), we might suspect that they react more strongly to immigrants than older workers with families settled and wage profiles plateauing. The entries in Table 3.6 generally confirm these considerations. Although we find no effects for workers below 30, looking at ages 30 to 49, we find that a one percentage point increase in local immigrant employment reduced native wage and employment growth by about 0.98% and 2.11%.[42] Old workers, in contrast, respond to the same inflow only on the employment margin (-1.93%), though as we illustrate below, much of this reduction arises through increased outflows into unemployment, which, at the time, was an attractive path to early retirement.[43]

Simple and Advanced Occupations: Since the immigrants that we consider were less educated on average than natives, and spoke the German language at lower levels of proficiency, we might expect the effect on natives to be stronger in low skill jobs (e.g., cleaning) than high skill jobs (e.g., planning, managing, or designing). To test this hypothesis, we slice the sample into simple and advanced occupations based on the task composition associated with each job. Using the 1985 wave of the BIBB/IAB Qualification and Career Survey (see Appendix B.1), we classify the following tasks as "advanced": designing, making plans, restoring, servicing and equipping machines (Prantl and Spitz-Oener, 2014). We then define

[41]Somewhat ironically, it is precisely this large employment decline in affected regions which may have shielded unskilled workers staying employed from incurring similar wage cuts as skilled.

[42]Note that the joint occurrence of negative wage *and* employment effects for the large group of middle-aged workers is consistent with the local supply of capital not being fully elastic (DSS).

[43]DSS report the largest employment effects for old, followed by young and middle aged workers. Our results show the largest response among middle aged workers.

Table 3.6: Effects of Immigration on Native Wage and Employment Growth by Subgroups

	Wages (1)	Employment (2)
Pooled Sample	-0.677	-1.125
	(0.281)	(0.718)
Panel A: Age Groups		
Below 30	-0.650	0.892
	(0.471)	(0.947)
Between 30 and 49	-0.983	-2.109
	(0.305)	(0.777)
50 and above	-0.247	-1.927
	(0.352)	(0.957)
Panel B: Occupational Complexity		
Simple	-0.952	-1.169
	(0.316)	(0.810)
Advanced	-0.192	-0.919
	(0.303)	(0.887)
Panel C: Sectors		
Tradable	-0.506	-2.054
	(0.284)	(0.708)
Nontradable	-1.041	0.701
	(0.369)	(0.898)
Panel D: Gender		
Men	-0.651	-1.179
	(0.302)	(0.940)
Women	-0.713	-0.984
	(0.334)	(0.606)
Local labour markets	204	204

Note: Table shows cumulative wage and employment effects from a series of models relating native annual wage and employment changes to the aggregate inflow of immigrants between 1988 and 1993, instrumented using distance to border. Two-stage least squares estimations are implemented in two steps, where the first stage is weighted by initial native employment in 1988, and the second stage is weighted by native employment in the base year. Estimates are trend-adjusted using a region-specific linear trend based on the 1986-1988 period. Standard errors are calculated using a pairs bootstrap with 1,000 replications. *Source:* SIAB 7510

"job complexity" as the average share of advanced tasks in an occupation, and consider an occupation as advanced (simple) if the associated share is above the employment weighted median of the job complexity index. By design, simple occupations thus contain relatively high routine and manual task shares that can be easily performed by lesser skilled immigrants. As panel B of Table 3.6 shows, we find no effects in advanced jobs, but a large and significant wage depression of 0.95% in simple occupations — corroborating our expectation that the impact on natives is larger in jobs more likely to be performed by immigrants.

Tradable and Non-Tradable Industries: In a recent study for Germany, DG show that an inflow of immigrants has significant distributional effects on resident employment. Specifically, using variation between regions and skill groups, they find that an inflow of immigrants reduces the relative wage of workers in the non-tradable sector, but has no effect in the tradable sector.[44] In contrast to their empirical approach, we only exploit regional variation in immigration shocks, hence we can examine what type of native response in each sector generates the distributional effects. We investigate this in panel C of Table 3.6. A striking observation is that the wage impact in non-tradable industries is about twice as large as in tradable industries, whereas employment effects are entirely concentrated in the tradable sector. This means that the relative wage effect reported in DG is associated with an overall decline in average wages in the non-tradable sector of affected areas relative to unaffected ones, consistent with firms in the tradable sector responding to changes in labour supply primarily by means of technological adjustments (instead of wages). It is natural to wonder what provokes these markedly different response margins in the two sectors. One explanation might be that wages in the tradable sector are set at the industry level, implying that the local relative wage elasticity in response to immigration is low.[45] While beyond the scope of this paper, we believe that inquiring deeper into this heterogeneity, perhaps by incorporating the dimension of product and labour market regulation (Prantl and Spitz-Oener, 2014), is a promising avenue for future research in this area.[46]

3.6.4 Robustness

In this section, we show that our main results are robust to a variety of robustness checks regarding the possibility of correlated shocks, the unit of observation, the selection of regions, the inclusion of further covariates, and alternative measures of the immigration shock and native outcomes. We moreover illustrate that worker selection into nonemployment generates an upward bias in standard cross-sectional wage estimates often applied in the immigration literature. Having established that our results hold up under these alternatives, we then turn to investigate in more detail the different response margins generating the overall employment response of natives.

Correlated Shocks: The causal interpretation of our IV estimates hinges on the identifying assumption that distance to border is uncorrelated with other shocks affecting local native outcomes. The combination of region-specific linear trends and cleaned outcomes accounts for pre-existing structural differences in wage and employment growth as well as contemporary differences in the demographic structure possibly correlated with immigration and native outcomes. We also showed in Figure 3.5 that distance to border has no effect on native outcomes prior to 1988. However, it is conceivable that our instrument is correlated with shocks associated with German reunification, which only happened after 1988. For

[44] Using the terminology of Dustmann et al. (2016), DG's design is based on the mixture approach, which uses variation in immigration across regions and skills cells, thus identifying these distributional effects of immigration. We instead use the pure spatial approach, exploiting only variation in the immigration shock across regions.

[45] DG investigate this by looking at union coverage rates, concluding that industry level wage setting is probably not the main source.

[46] As shown in panel D, we also looked for heterogeneous effects across genders. Although a large literature shows that female labour supply is on average more elastic than male labour supply (see Evers et al., 2008, for a review), we do not find a larger employment effect for women than men.

Table 3.7: Wage and Employment Effects of Immigration Under Alternative Model Specifications

		Correlated Shocks					Unit of Observation		Selection of Regions		Additional Covariates			
	Base-line	Exclude subsidised districts	Baseline + Bartik demand IV	Distance to border > 80 km	High native inflows	Exclude building sector	District level	Individ. level	Highly exposed	Large labour markets	Unad-justed	Col. 11 + demogr. controls	Col. 12 + Bartik demand IV	Long difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Wages														
All	-0.677	-0.680	-0.761	-0.681	-0.627	-0.639	-0.643	-0.757	-0.287	-0.839	-1.260	-0.956	-0.660	-0.638
	(0.281)	(0.325)	(0.259)	(0.278)	(0.256)	(0.297)	(0.247)	(0.241)	(0.456)	(0.337)	(0.375)	(0.595)	(0.476)	(0.275)
Unskilled	-0.695	-0.512	-0.301	-0.595	-0.655	-0.691	-0.723	-0.748	-1.011	-1.718	-1.092	-2.172	-0.989	1.177
	(0.459)	(0.539)	(0.411)	(0.506)	(0.511)	(0.450)	(0.412)	(0.473)	(0.831)	(1.550)	(0.461)	(1.224)	(0.932)	(0.622)
Skilled	-0.581	-0.610	-0.800	-0.605	-0.529	-0.517	-0.536	-0.687	-0.088	-0.781	-1.331	-0.793	-0.776	-0.914
	(0.294)	(0.335)	(0.267)	(0.302)	(0.253)	(0.316)	(0.249)	(0.244)	(0.423)	(0.353)	(0.372)	(0.587)	(0.497)	(0.296)
Local labour markets	204	272	204	144	204	204	325	204	100	94	204	204	204	204
Individuals								448,604						
Panel B: Employment														
All	-1.125	-1.459	-0.953	-1.793	-1.048	-1.224	-0.950	-0.398	-1.509	-2.302	-2.497	-0.919	-0.650	-1.617
	(0.718)	(1.032)	(0.538)	(0.691)	(0.716)	(0.740)	(0.794)	(0.301)	(0.896)	(0.548)	(0.762)	(1.462)	(0.904)	(0.626)
Unskilled	-2.610	-2.427	-2.742	-2.319	-2.466	-2.420	-2.366	-1.849	-1.723	-3.914	-3.554	-2.263	-1.998	-3.153
	(1.166)	(1.308)	(1.117)	(1.149)	(1.140)	(1.211)	(1.185)	(0.600)	(1.676)	(3.865)	(1.403)	(2.636)	(1.949)	(1.149)
Skilled	-0.917	-1.332	-0.631	-1.786	-0.850	-1.049	-0.739	-0.107	-1.485	-2.170	-2.530	-1.045	-0.273	-1.511
	(0.779)	(1.050)	(0.643)	(0.816)	(0.819)	(0.784)	(0.884)	(0.309)	(0.991)	(0.611)	(0.811)	(1.546)	(1.123)	(0.686)
Local labour markets	204	272	204	144	204	204	325	204	100	94	204	204	204	204
Individuals								530,282						

Note: Tables reports estimates of the wage and employment effect of immigration between 1988 and 1993 for different types of regions and under alternative model specifications. Column 1 reports the baseline results for reference. Columns 2-6 analyse sources of correlated shocks by imposing different restrictions on the sample. Columns 7 and 8 change the level of observation from commuting zones to the district level and to the individual level. In columns 9 and 10, we estimate the models for different types of regions. In columns 11-13, we use unadjusted native wages and employment (see main text for the adjustment procedure), and successively include additional control variables. Column 14 reports coefficients from models based on the long difference, i.e., without conditioning the wage analysis on workers being present in two consecutive periods. All models include a linear region-specific trend for years 1986-1988. Except for column 8, standard errors are bootstrapped using a pairs bootstrap with 1,000 replications. In column 8, standard errors are clustered at the commuting zone level.
Source: SIAB 7510

example, increased market access (Redding and Sturm, 2008), the phasing out of subsidies to the former border region (*Zonenrandförderung*), or industrial relocation (Redding et al., 2011) might be functions of distance *and* correlate with native outcomes.[47] We address these concerns in two ways reported in columns 2 and 3 of Table 3.7. First, we exclude regions receiving border zone subsidies prior to 1994.[48] Second, we augment our baseline model with a Bartik instrument to control for coincident demand shocks (Bartik, 1991). Specifically, we predict the 1988-1993 native wage (employment) growth in a region based on its industry structure in the base year and the industry-specific wage (employment) growth in all other regions. Reassuringly, these exercises yield very similar wage and employment effects as our baseline estimates, suggesting that our results are not driven by correlated shocks in the aftermath of reunification.

As noted above, we might still be concerned with confounding effects of East and ethnic German inflows. One approach would be to examine whether the results are robust to the exclusion of regions near the inner German border. Indeed, excluding all areas whose outer contour reaches into an 80 km strip from the former inner German border does not affect our estimates (column 4). Another approach would be exclude regions with unusually high native inflows, assuming that these must be driven by East and ethnic Germans. We do so in column 5, again finding very similar effects as in the baseline model.

A final concern regards the possibility of understating the actual immigrant employment shock (and thus overestimating the immigration effect) since we only observe workers showing up in the social security system. Indeed, the period under consideration has seen a massive rise in labour migrants through the implementation of bilateral labour treaties (section 3.2), and these workers were generally not subject to social security contributions in the host country.[49] While it is difficult to fully account for this effect, we try to at least partially address this concern by excluding the building sector from the analysis, which has been the key employer of labour migrants. As shown in column 6, we find no evidence of an undercounting bias.

Unit of Observation: Our main analysis is based on 204 commuting zones, representing aggregates of 325 districts. To test whether our results depend on the particular regional unit, we re-estimated our baseline wage and employment models at the district level rather than the commuting zone. The associated coefficients are reported in column 7, and they are very similar to our baseline estimates. Another specification check is to investigate the impact of immigrants directly on the worker level. To this end, we calculate first differences

[47] It is *a prioi* unclear whether these shocks would lead to an upward or downward bias in our estimates. On the one hand, increased market access for border regions after reunification would imply an upward bias. On the other hand, the parallel phasing out of substantial subsidies for border regions until 1994 would suggest a downward bias.

[48] These are detailed in the Federal Law Gazette (*Bundesgesetzblatt*) 77, pp. 1217-1240 (1971). Estimations are based on the district-level.

[49] The number of labour migrants rose by 80,000 workers between 1988 and 1993, whereas the total number of social security employed immigrants rose by 450,000 workers. This suggests that we might overstate the impact of immigration by about one-fifth.

in wages and employment for each worker (and within regions in the case of wages), and regress the trend-adjusted change in each outcome on the instrumented immigrant inflow. We control for gender interacted with full sets of age and education dummies, and cluster standard errors at the regional level. As shown in column 8, point estimates and significance levels line up well with our baseline results.

Selection of Regions: One might be concerned that our results are driven by particular types of regions, e.g., the largest or smallest regions, or regions with particularly high immigrant exposure. To investigate this, columns 9 and 10 of Table 3.7 repeat our analysis for three different types of regions. First, we consider only regions with very high immigrant exposure, defined as an above median percent increase in immigrant employment between 1988 and 1993. Despite some variation in magnitude and significance in both outcomes, we find consistently negative signs and cannot detect systematic deviations from our baseline estimates. Second, we restrict the sample to large regions with an average labour force exceeding 50,000 individuals. As shown in column 10, both wage and employment effects are more pronounced in this subsample. One reason might be that large regions typically feature higher average wage and employment growth, which generates additional leeway for reductions in the associated growth rates due to immigration.

Additional Covariates: As explained above, we residualise native wages and employment in each year before calculating region level aggregates. However, since some native characteristics might themselves be endogenous — for example, facing an immigrant shock, native labour market entrants might decide to study one more year rather than compete with unskilled immigrants for jobs (Hunt, 2017) — it may be preferable to condition on pre-shock characteristics instead. We analyse this in columns 11-13 of Table 3.7, starting from a specification with raw instead of cleaned outcomes and no controls except for a linear trend, and then augmenting these models with region level covariates averaged over the 1986-1988 period. The results in column 11 suggest somewhat larger wage and employment effects than our baseline results, indicating an overall average decline of 1.26 and 2.5%, respectively. In column 12, we add an array of region level covariates: the shares of middle and old workers, the fraction of females, the share of advanced occupations, the share of tradable industries, the overall employment level, and the unemployment rate. Overall, the results are now much closer to our baseline estimates both in terms of wages and employment, with the only notable deviation remaining for the small group of unskilled workers. Finally, in column 13, we also add a Bartik instrument (see above). This has two effects: on the one hand, it brings our wage estimates even closer to our baseline results (also for unskilled); on the other hand, the employment response shrinks to about 50% of our baseline coefficient, mainly driven by skilled workers. Overall, however, the patterns look very similar to our baseline estimates.

Worker Selection: Most studies of the effects of immigration rely on repeated cross-sections to estimate the response in native wages to an immigration shock (Card, 2001; Glitz, 2012). Only a few recent studies such as Bratsberg and Raaum (2012), Foged and Peri (2016), and

Table 3.8: Comparison of Different Measures of Immigrant Inflows and Native Outcomes

		Employment			Wages
	Type of Standard Error	%-change native employment o/ employment (baseline)	%-change native employment o/ labour force	%-change native employment o/ population	%-change native wages (baseline)
	(1)	(2)	(3)	(4)	(5)
Panel A: Instrument: South and East Distance to Border					
%-change in immigrant employment share		-1.125	-1.319	-0.368	-0.677
	Pairs	(0.718)	(0.621)	(0.208)	(0.281)
	2SLS	(0.700)	(0.575)	(0.202)	(0.247)
First stage F-statistic		16.43	16.51	16.71	16.43
%-change in immigrant labour force share		-1.319	-1.247	-0.417	-0.879
	Pairs	(0.621)	(0.556)	(0.186)	(0.234)
	2SLS	(0.575)	(0.549)	(0.173)	(0.215)
First stage F-statistic		28.33	28.61	28.57	28.33
%-change in immigrant population share		-1.696	-1.623	-0.514	-1.424
	Pairs	(0.530)	(0.515)	(0.162)	(0.378)
	2SLS	(0.555)	(0.530)	(0.167)	(0.296)
First stage F-statistic		18.25	18.47	21.73	18.25
Local labour markets		204	204	204	204
Panel B: Instrument: Population Shift Share (Base Density 1961)					
%-change in immigrant population share		-2.708	-2.529	-0.816	-1.313
	Pairs	(0.542)	(0.506)	(0.158)	(0.289)
	2SLS	(0.768)	(0.754)	(0.261)	(0.456)
First stage F-statistic		*32.56*	*31.22*	*30.24*	*32.56*
Local labour markets		112	112	112	112
Weight (as of 1988)		native employment	native labour force	native population	native employment

Note: Table shows cumulative native wage and employment outcomes for alternative measures of native employment (columns 1-3) and immigrant shocks (across rows) between 1988 and 1993. Columns 1 and 4 are based on the baseline definition of native employment and wage changes. Column 2 scales employment changes by the native labour force, and column 3 by native population. All models refer to the pooled regressions, combining skill and age groups. We weight the first stage by total native employment (columns 1 and 4), labour force (column 2), and population (column 3) in 1988, and the second stage by the corresponding values in the previous year. All models include a linear region-specific trend for years 1986-1988. The first row measures the immigrant shock by the percentage change in the immigrant employment share; the second row, by the percentage change in the immigrant labour force; and the third row by the percentage change in the immigrant population. Standard errors are bootstrapped using a pairs bootstrap with 1,000 replications. F-Statistics for instrument excludability are reported below standard errors.
Source: SIAB 7510

DSS exploit longitudinal worker spell data to account for differential worker selection into nonemployment. If low wage workers are more likely to select into nonemployment, simple cross-sectional comparisons produce upward biased wage effects. To investigate this, we report in column 14 of Table 3.7 estimates based on regional wage changes calculated as the difference in mean wages between all workers in 1993 and all workers in 1988.[50] We find considerably larger coefficients for unskilled workers, with point estimates rising from an insignificant -0.695 to a significant 1.177, confirming the DSS findings and underpinning the importance of controlling for selection effects.[51]

[50]In these models, we use a manual trend-adjustment, i.e., we subtract the average value of the outcome

Table 3.9: Impact of Immigrants on Native Inflows/Outflows and Geographic/Nonemployment Flows

	Inflows				Outflows				Net Flows (= Inflows – Outflows)			
	Geo-graphic (1)	Nonem-ployment (2)	Unem-ployment (3)	Sum of Inflows (4)	Geo-graphic (5)	Nonem-ployment (6)	Unem-ployment (7)	Sum of Outflows (8)	Geo-graphic (9)	Nonem-ployment (10)	Unem-ployment (11)	Net Em ployment (12)
Total employment flow	-0.67	-0.28	0.36	-0.59	0.05	-0.36	0.85	0.54	-0.72	0.09	-0.49	-1.13
	(0.43)	(0.41)	(0.25)	(0.64)	(0.41)	(0.32)	(0.40)	(0.53)	(0.39)	(0.53)	(0.37)	(0.71)
Panel A: Skill Groups												
Unskilled	-0.09	-1.04	-1.04	-2.17	0.11	-0.72	0.72	0.44	-0.20	-0.32	-1.77	-2.61
	(0.33)	(0.92)	(0.41)	(0.94)	(0.45)	(0.75)	(0.50)	(0.90)	(0.40)	(0.94)	(0.70)	(1.15)
Skilled	-0.79	-0.04	0.65	-0.23	0.09	-0.24	0.84	0.69	-0.88	0.20	-0.19	-0.92
	(0.47)	(0.46)	(0.27)	(0.82)	(0.38)	(0.33)	(0.42)	(0.52)	(0.44)	(0.61)	(0.35)	(0.80)
Panel B: Age Groups												
below 30	-2.30	1.16	1.70	0.56	-0.73	-0.93	1.67	-0.33	-1.57	2.09	0.03	0.89
	0.66	(0.75)	(0.54)	(1.16)	(0.55)	(0.54)	(0.59)	(0.88)	(0.65)	(0.74)	(0.44)	(0.95)
between 30 and 49	0.07	-1.25	-0.28	-1.43	0.54	0.26	-0.14	0.68	-0.46	-1.51	-0.14	-2.11
	(0.47)	(0.41)	(0.25)	(0.59)	(0.39)	(0.44)	(0.30)	(0.60)	(0.46)	(0.59)	(0.37)	(0.75)
50 and above	0.09	-0.45	-0.43	-0.76	0.16	-0.50	1.75	1.17	-0.07	0.05	-2.19	-1.93
	(0.38)	(0.35)	(0.31)	(0.52)	(0.32)	(0.66)	(0.67)	(0.85)	(0.30)	(0.73)	(0.72)	(0.96)
Panel C: Gender												
Men	-0.63	-0.38	0.68	-0.33	0.27	-0.63	1.21	0.85	-0.90	0.26	-0.53	-1.18
	0.54	(0.46)	(0.25)	(0.74)	(0.40)	(0.36)	(0.32)	(0.57)	(0.60)	(0.60)	(0.36)	(0.94)
Women	-0.75	-0.06	-0.10	-0.91	-0.29	0.02	0.34	0.07	-0.46	-0.08	-0.44	-0.98
	(0.36)	(0.51)	(0.23)	(0.74)	(0.37)	(0.50)	(0.49)	(0.69)	(0.28)	(0.64)	(0.43)	(0.67)

Note: Table shows estimates of the effect of changes in immigrant employment between 1988 and 1993 on native inflows and outflows. Columns 1-4 show inflow rates, columns 5-8 outflow rates, and columns 9-12 show net flows. Columns 1, 5, and 9 refer to geographic mobility which is identified from individual worker spells if a person is employed in two different regions in two consecutive years. Columns 2, 6, and 10 refer to nonemployment flows, defined as changes from unobserved into employment or from employment into unobserved. Columns 3, 7, and 11 refer to unemployment flows, defined as changes from observed unemployment into employment, and vice versa. Columns 4, 8, and 12 report the sum of all flow components. All models include a linear region-specific trend for years 1986-1988. Bootstrapped standard errors are reported in parentheses and calculated using a pairs bootstrap with 1,000 replications.
Source: SIAB 7510

Alternative Measures of Immigration Shocks: Our definition of the native employment response (dependent variable) and our measure of the immigration shock (independent variable) differ from specifications typically used in the immigration literature. Specifically, we standardise the change in local native employment by native *employment* in the base year, rather than dividing by the labour force or population (Altonji and Card, 1991; Pischke and Velling, 1997; Dustmann et al., 2005). In addition, our immigration shock is measured in terms of employment as opposed to, e.g., labour force or population inflows. To explore the sensitivity of our results against these alternatives, we collected data on native and immigrant populations at the district-level, enabling us to scale the employment change (ΔE) not only by employment (E, our baseline) but also by labour force (LF) and population (P). We also built two additional versions of our immigration shock variable, measuring the inflow either in terms of the labour force or in terms of the population.[52] We then regressed each native employment proxy (ΔE/E, ΔE/LF, ΔE/P) and the wage change on each of the three immigrant shock variables, instrumented using distance to border.

Table 3.8 reports the results, showing different employment measures from left to right, and different immigrant shock measures from top to bottom. Reading across columns thus tells us how a different scaling of the native employment response changes the estimated coefficients (conditional on how we measure the immigration shock), whereas reading across rows tells us how different ways of gauging the immigration shock affect our conclusions (conditional on how we measure the native employment response). For each combination of dependent and independent variable, we display cumulative coefficients (1988-93), standard errors, and F-Statistics for the first stage. Moving down column 1, we see that our baseline results (row 1; compare Table 3.5) hardly change if we measure the immigration shock in terms of labour force (row 2) or population (row 3) instead of employment. Moving across row 1, we observe a relatively stable effect for the labour force measure but a sizeable drop by about two-thirds when measuring the employment change by the employment-to-population ratio (similar patterns emerge in rows 2 and 3). Hence, while the definition of our dependent variable might explain why we report larger employment effects than typically found in the literature, our particular definition of the immigrant inflow appears rather innocuous — if anything, our approach delivers conservative estimates. In the last column, we report the associated wage effects for each immigration proxy, again suggesting that our baseline estimates represent a lower bound.

As a final check, we collected population data for the earliest year available, 1961, to construct a standard shift share instrument based on historical immigrant population densities.[53] When we use this instrument instead of distance to border, we obtain the highest

variable for each region over the years 1986-1988 from the dependent variable; see section 3.4.

[51] For employment, in contrast, net changes are relatively similar, with differences arising mainly through approximation errors and different weighting factors.

[52] To build the population series, we digitised Statistical Yearbooks (years 1985-1990) as well as multiple versions of the BBSR Laufende Raumbeobachtung (years 1986, 1989/90, 1992/93).

[53] The Stata file is available online at GESIS data archive, file name ZA2472. It provides, among others, census data for years 1961 and 1987. Bavaria and North Rhine-Westphalia are not included, which explains the

F-statistics, and also larger and more precisely estimated wage and employment coefficients, reinforcing our conclusion that the short run effects of immigration reported above tend to be conservative (see also Appendix B.5).

3.6.5 Margins of Adjustment: Inflows vs. Outflows and Geographic vs. Nonemployment Flows

Our results reveal substantial employment adjustments due to immigration for unskilled workers, middle and older age groups, and, more broadly, in the tradable sector. These findings raise three questions which we set out to answer in this section. First, are these effects generated by increased employment outflows, reduced inflows, or both? Second, are these flows emerging between regions (within employment), or rather between employment and nonemployment? Third, do the insignificant employment effects found for certain subgroups potentially conceal a rise in overall worker flows, with growing inflows and outflows compensating each other? To answer these questions, we decompose the net employment effects into outflows from and inflows into employment, distinguishing between two types of transitions: geographic transitions that lower employment in one region and increase employment in another; and nonemployment transitions that lower employment in one region without a corresponding gain in another region. We further decompose the nonemployment transitions into transitions from and to unemployment (if a person is observed as unemployed prior/post an employment spell) and transitions from and to nonemployment (if a person is not observed prior/post an employment spell).

The results for several subsamples are presented in Table 3.9. Columns 1-4 refer to inflows, columns 5-8 to outflows, and the last four columns to net flows, i.e., the difference between inflows and outflows. In each set of columns, we show, from left to right, geographic, nonemployment, and unemployment transitions, followed by the total effect in the last column — column 12 corresponds to the total employment effect reported in Tables 3.5 and 3.6.

Inflows and Outflows: The entries in columns 4 and 8 show the total contributions of inflows and outflows, respectively. Overall, we find that the negative employment effects of natives are generated by a combination of reduced inflows into employment and higher outflows from employment. For example, using the average coefficients for the three groups with significant employment declines (unskilled, ages 30-49, and 50+), we find that inflows account for about two-thirds (1.45%) and outflows for about one-third (0.76%) of the overall average employment decline of about 2.21% in these groups — the importance of inflows for the employment response is in line with DSS, but contrasts with evidence in Wozniak and Murray (2012) who find the main response margin of natives to be outflows. In sum,

reduced number of observations. We combine this file with population data from the Federal Statistical Office for years 1985-2001.

our findings suggest that employment losses from immigration are, at least in the short run, induced by employers adjusting their hiring rate or poaching behaviour; that is, incumbent workers are protected from immigration effects at the expense of outsiders, possibly, because the latter feature a more elastic labour supply.

Geographic and Nonemployment Flows: In columns 9-11, we next partition the employment decline (column 12) into three terms gauging the contributions of geographic transitions (job-to-job), movements between employment and nonemployment, and movements between employment and unemployment. A striking conclusion from this exercise is the important role of geographic mobility in the pooled sample, suggesting that on average 64% (0.72/1.13) of the 1988-1993 employment decline in one region is compensated by employment gains in other regions. This means that, at the macro-level, the effect of immigration on native employment is considerably less negative than suggested by our region-level estimates. While the prominence of geographic mobility for the local native employment response is qualitatively in line with DSS, our estimate is about twice as large as theirs. One explanation might be that our immigrant shock was less clustered, meaning that natives had to move a shorter distance to avoid it. Moreover, our sample consists of many urban areas which, on average, feature higher labour mobility rates than the rural areas considered in DSS. A visual impression of the underlying adjustment processes is provided in Figure 3.6. While we find an immediate and sustained decline in geographic net flows (panel A), the contribution of nonemployment flows (panel B) is zero until 1993, and tends to increase thereafter. Unemployment flows (panel C), on the other hand, show an initial decline until 1992, and a return to zero in subsequent years. Overall, these results suggest that geographic mobility is a primary means of equilibrating local labour markets (Blanchard and Katz, 1992; Cadena and Kovak, 2016), with a sustained reduction in affected areas creating new job opportunities for nonemployed and unemployed workers in the longer run.

In panels A and B of Table 3.9, we break out the overall effect into different skill and age groups. For both skilled and young workers, geographic mobility exhibits sizeable declines — among skilled, it virtually accounts for the entire reduction in local employment growth. In contrast, among unskilled workers and workers in older age groups, a reduction in nonemployment and unemployment flows dominates the overall employment effect.

To investigate the sources of these effects, we further decompose the contribution of each channel into inflows and outflows (columns 1-3 and 5-7). Considering first geographic flows, the overall picture is one of a decline in inflows rather than an increase in outflows, that is, instead of workers in affected regions seeking employment elsewhere, it is workers elsewhere no longer moving into affected regions. Not surprisingly, this is especially true for skilled and younger workers for whom geographic flows are of particular importance. This result contrasts with evidence for the US (Wozniak and Murray, 2012), where immigrants increase outflows of high skilled natives, however, it is consistent with evidence for Germany (DSS), suggesting that geographic flows are primarily driven by the inflow margin. For nonemployment and unemployment transitions, we also find reduced inflows, especially among unskilled and middle-aged workers, but here outflows likewise play a role, most importantly in the youngest and oldest age groups. Looking at young workers in more detail, we note that the growing outflow into unemployment is somewhat compensated by a corresponding rise in inflows, implying an increase in gross native worker flows. One interpretation of this result is that immigration leads to a reshuffling of extant employment matches which would be consistent with a process of occupational specialisation as suggested by Peri and Sparber (2009), and might constitute another means of alleviating wage losses.

Finally, turning to older workers, we find that the growing outflow into unemployment is the main source of employment adjustments. We suspect that this outflow effect is amplified by one particular feature of the social security system at the time, which rendered unemployment for male workers approaching retirement a very attractive and often used choice. Specifically, conditional on at least 52 weeks of registered unemployment, male workers were allowed to retire up to 5 years early. Though suggesting at most, the annual growth rate of men in West Germany exploiting this option rose from 0.5 percentage points per year in 1970-1990 to 2 percentage points in 1990-1995. In 1995-2000, i.e., after the immigration shock, this rate returned to 0.7 percentage points (German Pension Fund, 2017).

3.7 Conclusions

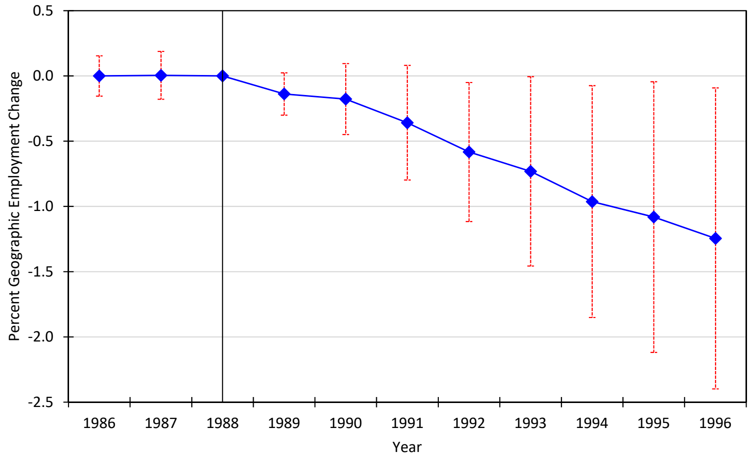
Research on the effects of immigration on native workers is abundant, yet a central concern, namely, whether immigrants reduce or raise native wages and employment remains a controversial topic: empirical evidence covers the whole range from negative effects, to zero and positive effects, even when analyses are based on the same time period, country, and data. Using detailed longitudinal administrative data, we provide the first comprehensive analysis of the short-run effects of a large and unexpected immigration shock between 1988 and 1993 on native wages and employment in the West German labour market.

Our analysis of local labour markets shows that immigrants tend to reduce native wages and employment by about 0.68% and 1.13% in the first five years after arrival, but tend to have no or even positive effects in the longer perspective. We find important heterogeneity across various groups of the labour market, which might be explained by different labour supply elasticities or wage rigidities. We also show that, while wage reductions are indeed borne by incumbent workers, i.e., those who are and stay employed in a given labour market, about two-thirds of the local employment losses are compensated by employment gains in other regions, meaning that those workers who move out of an affected region or those who no longer enter it often find employment in other regions. We show that the latter component is a key driver of the impact of cross-regional flows.

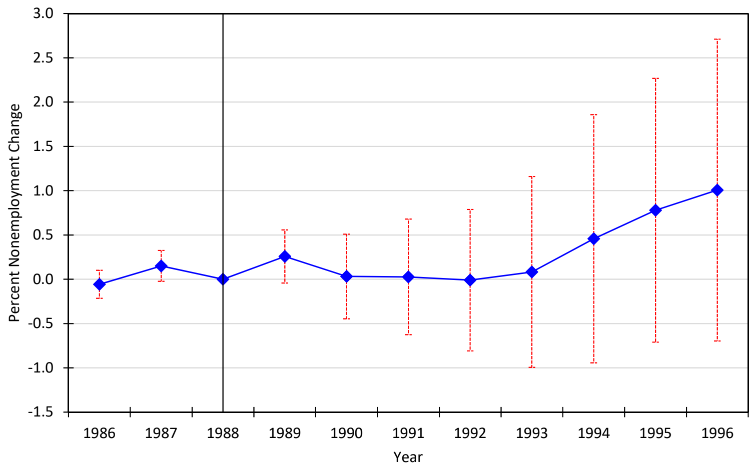
What do our findings imply for the current refugee debate? Latest since 2015, the topic of immigration has resumed centre-stage in the political debate in virtually all receiving countries in Europe, with populist parties rapidly gaining voter approvals (Dustman et al., 2016). At first glance, our results seem to spur those populist calls for more isolation, increased border control, and vetting of immigrants. But this is only half the story for two main reasons. First, the composition of source countries in the 1980/1990 refugee wave differed considerably from today's refugee inflow. Crucially, the inflow that we consider, comprising of many East Europeans and Yugoslavs (with guest worker background), was probably more educated and in a sense "culturally closer" than the composition of refugees arriving today. Consequently, the labour market impact of the current inflow would be expected to be less pronounced. Second, the specific conditions on the German labour market during our analysis period differed dramatically from today's situation. In the early 1990s, Germany was characterised by high and increasing stocks of unemployment, struggling with reunification, rigid labour market institutions, and a lack of international competitiveness. All these features prevented the German labour market from a speedy adjustment to the additional supply of labour, and likely contributed to the temporary decline in native wages and employment. However, our results suggest that even in this difficult situation, the long term effects of immigration were, if anything, positive. Looking at Germany today, the picture has completely reversed: due to trade unions' continued accommodation of moderate wage growth and rising wage flexibility, and due to substantial reforms of the social security system, the German economy has risen its productivity (measured in unit labour costs) and by now belongs to one of the most productive economies in the world (Dustmann et al., 2014), with unemployment rates chasing one record low after the other. Against this backdrop, the German economy seems ready to leverage the opportunities associated with the inflow of immigrants in order to keep fueling the booming economy. To accomplish this, a fast processing of asylum applications and parallel integration into the labour market is imperative.

Figure 3.6: Decomposition of the Cumulative Net Employment Effects Into Geographic and Nonemployment Flows

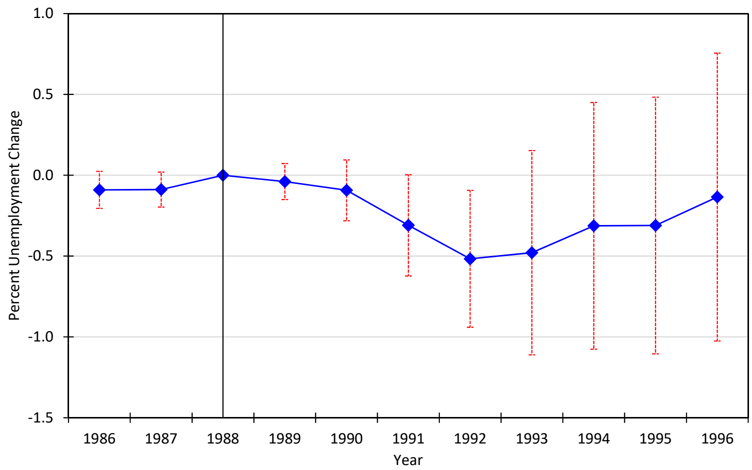
A. Cumulative Geographic Net Flows



B. Cumulative Nonemployment Net Flows



C. Cumulative Unemployment Net Flows



Note: Figure shows a decomposition of the cumulative net employment flows for years 1986-1996 from Figure 3.5B into geographic, nonemployment, and unemployment flows. In each year, we plot the sum of coefficient estimates relative to 1988, that is, we sum backward and forward. 95-percent confidence bands are indicated in red. The vertical line represents the start of the immigration shock.
Source: SIAB 7510

4 Finding Your Right (or Left) Partner to Merge

4.1 Introduction

The organisation of the state is in constant reform. In many countries, there are ongoing debates how to allocate the responsibilities between governmental tiers, and how to reform tier structures to form more effective units. Overall, researchers have identified a trend towards decentralisation of spending and taxing authority along with a tendency of merging multiple (local) administrative units to larger entities.[1] Dating back to Oates (1968), economists see the relevant economic tradeoff between local preferences and cost efficiency. Local representation is likely to understand the preferences and needs of the constituency better than a central government will. On the other end, an entity of a crucial size can capitalise on economies of scale and provide public goods in the most cost efficient way (Alesina and Spolaore, 1997; Ellingsen, 1998). While this tradeoff is certainly relevant, a growing literature has recognised the role of political decision makers in the reform processes, and has started to investigate the political economy behind the merger decision making (Saarimaa and Tukiainen, 2014; Hyytinen et al., 2014).

This paper studies a reform of municipal boundaries in the German state of Brandenburg. Between 1999 and 2003 the number of municipalities in the state decreased by approximately two thirds from 1,489 down to 421. Our principal goal is to analyse the role of the political decision makers during this merger reform. We analyse whether politicians pursue strategies that would benefit their private interest by making it more likely for them to gain office in the newly established municipality, or at least ensuring that the political leadership in the new structure is of their liking. Specifically, we ask whether voluntary mergers are more likely when the party structure in the town councils is comparable.

The reform was implemented in two stages. In the first stage, the state legislator gave out a set of targets that the municipalities were supposed to meet. The municipalities were then given about two years time to meet the targets through voluntary amalgamation, which we denote the voluntary stage of the reform. It is this stage, at which the decision of whom to merge with was potentially influenced by political considerations. In the second stage, the municipality structure was reviewed by the state government and a law was passed which

[1] For example, during the 20th century, the number of American municipalities decreased by more than 90 percent (Alesina et al., 2004). Similar processes have occurred throughout the developed world. Sweden reduced the number of municipalities by more than 2,500 (Tyrefors Hinnerich, 2009; Jordahl and Liang, 2010), and West Germany reformed the municipality boundaries to reduce the number by about 33,000 (Sancton, 2000).

amalgamated municipalities at the discretion of the state authorities. Here, local politicians had no say in the amalgamation process.

The literature on municipal mergers has been interested in analysing certain aspects of similar reforms. For Sweden, studies by Tyrefors Hinnerich (2009) and Jordahl and Liang (2010), for instance, focus on the incentives for free riding behaviour preceding an amalgamation.[2] A study on a municipality reform in Japan by Weese (2015) estimates a structural model of optimal municipality size, taking the desired population number and local government efficiency into account. A recent study for Israel (Reingewertz, 2012) studies the efficiency aspects of boundary reforms, and finds significant economies of scale. Similarly two studies for Germany investigate the potential for such scale effects:[3] Fritz (2011) reports the surprising result that, during a major merger reform in Baden-Wuerttemberg in the 1960s-70s, mergers in fact increased overall expenditures; and Blesse and Baskaran (2016) find positive economies of scale for a major reform in Brandenburg, in fact, the same reform that we investigate in this work. They compare the fiscal outcomes of voluntary, forced and non-merged municipalities and report significant reductions in expenditures for the forcingly merged municipalities and less stable effects for voluntary mergers. Their result, thus, highlights important differences in the type of merger. Our research provides a potential explanation for their results. If political considerations (rather than efficiency arguments) drive the decision to merge voluntarily, this can explain why the potential for scale economies is smaller in voluntary mergers than in forced mergers.

Closest to our paper is a set of work by Saarimaa and Tukiainen (2014) and Hyytinen et al. (2014), studying amalgamations of Finnish municipalities that were induced by a newly created subsidy scheme. Similar to our paper, their objective is to analyse strategic behaviour of politicians in merger decisions. They find that political congruence is an important merger determinant (Saarimaa and Tukiainen, 2014), and that politicians seem to take individual concerns into account when deciding on the mergers (Hyytinen et al., 2014). These papers are interesting as data availability for Finland allows to study the individual voting behaviour of the politicians. At the same time, however, the analyses are limited to few actual mergers, and need to focus entirely on voluntary amalgamations — in contrast, our setting provides a large number of mergers, some of which occurred voluntarily while others were forced by state legislature. Further evidence on political determinants is somewhat mixed. While Strebel (2016) suggests that political effects are an important determinant in the merger process (for the case of Swiss towns in the canton of Fribourg), Bhatti and Hansen (2011) find that political aspects did not matter over and above geographical, socio-economic and demographic influences.[4]

[2]These common pool results have also been confirmed in a recent study for Finland, documenting significant increases in debt (and decreases in assets) before voluntary mergers; see Saarimaa and Tukiainen (2015).

[3]Further papers that investigate economic effects in merger reforms include, among others, Blom-Hansen et al. (2014) and Hanes (2015).

[4]Also note that Di Porto et al. (2016) find that political alignment does not matter much in the decision of towns to cooperate in the provision of public goods, which, however, is a notably different decision than to amalgamate municipalities entirely.

To conduct our analysis, we have built an extensive data set on all mergers and all participating municipalities in the boundary reform. For the latter, we collected pre-reform data on election results, including mayoral elections, detailed budgetary information, and important socio-economic variables. We argue that our information about the budgetary situation, in particular, puts us in a position of knowing as much about the potential merger partners as the actual decision makers themselves.

We propose to estimate a reduced form model of factors driving the merger decisions of municipalities. The decision about a voluntary amalgamation is treated as a binary outcome, indicating if a given municipality agrees on a particular merger (1), or not (0). We are interested in what determines the municipality's choice to form a voluntary merger in relation to the characteristics of the merger partners. In particular, we focus on the question whether politically aligned municipalities are more often observed to form a voluntary merger.

Since we observe a number of voluntary mergers, it is not obvious what the feasible and realistic counterfactual observations can be. To address this issue, we draw on two different research designs: First, we exploit the particular timing of the reform, and compare voluntary mergers to the set of forced mergers. These observations are, for obvious reasons, realistic amalgamations that potentially could also have happened in the first stage of the reform. However, since the state forced those mergers, they are unlikely to be motivated by political considerations (at least not by motivations of the local politicians). Second, we follow a new trend in the literature on coalition formation, and simulate counterfactual alternative coalitions (mergers) that could have occurred. Here, we draw potential mergers from the map of municipalities, and then study the specific characteristics of the actual to the simulated mergers (see Saarimaa and Tukiainen, 2014).

The approaches have their strengths and weaknesses. The comparison of voluntary to forced coalitions is interesting because all of those mergers are actual events. However, the forced mergers were all enacted in the second phase of the reform which takes the first phase as given. For the analysis of voluntary mergers compared to simulated ones it is quite the opposite. Here, the timing is such that we can simulate the potential mergers from the map of municipalities at the time when no mergers have yet been undertaken. We, thus, can ask what other merger options did the individual municipalities have. The assumption that we have to make here, however, is that we, as researchers, can actually simulate relevant merger options.

Both approaches allow us to analyse the determinants of voluntary merger decision. Importantly, both designs can abstract from pure spatial correlation in the political variables.[5] However, we must note that both designs are not in itself suitable to deal with potential omitted variable bias. For the estimation of a causal effect of political representation, we need to be concerned that our political variables simply proxy for other potential determinants of the merger decision. To evaluate the magnitude of this problem, we study the behaviour of our estimates when we include numerous variables that directly control for potentially

[5]This spatial correlation, which we can show exists in the data, would be the same for municipalities in voluntary and forced mergers as well as for simulated mergers.

correlated determinants. We argue that it is enough to control for those measures, as also the individual municipality had no more information about the budgetary and the socio-economic characteristics of the other potential merger partner.

Our estimation results from both designs show that there is a clear effect between the dominant party in the council of a municipality and the share of that party in the councils of partners that this municipality chooses to form a coalition with. These results are robust when we deviate from the simple model and include additional sets of control variables. We conclude that political considerations play a sizeable and significant role in the formation of mergers during the voluntary phase of the reform.

The study proceeds as follows. Section 2 explains the institutional background. The data set is presented in section 3, and the empirical methodology is introduced in section 4. The results of the estimation will be presented in section 5, before section 6 concludes the analysis.

4.2 Institutional Setting and the Timing of the Boundary Reform

4.2.1 Local Government in the State of Brandenburg

Government in Germany is organised in three major tiers: the federal government, 16 federal states, and the local level which is again organised in about 450 districts and some 12,500 municipalities. Apart from a strong federal involvement in all branches of state activity, there are specific responsibilities for each of the three tiers. Education, for example, is largely in the responsibility of each individual state, while counties are mostly concerned with issues of public order (police, fire rescue) and health (hospitals, ambulances). The municipal level has some direct responsibilities such as child care, cultural expenditures, sport and recreational facilities, and local infrastructure investments, and it often oversees public firms to deliver services (e.g., energy and water supply). At the same time, the municipality is often the institution that administrates mandated spending allocated by higher tiers (like social services, investment in schools, and certain infrastructural investments).

In the former German Democratic Republic (GDR), municipalities had very limited political power and only few administrative tasks to fulfil. As a consequence the municipality structure in former East Germany had never seen any reform under the Communist system while western communities went through a number of reforms (including large scale amalgamations) during the 1960s/70s (e.g., in Baden-Wuerttemberg, Bavaria and North Rhine-Westphalia). After the reunification in 1990 the West German constitution (*Grundgesetz*) was introduced in East Germany. It suddenly granted municipalities extensive rights and duties (see above) along the principle of local self-governance. Taking over those responsibilities created extensive challenges for all east German communities, yet especially for very small municipalities. They lacked the administrative resources to fulfil tasks they were required to do. A necessary reform of the municipality structure, however, was considered politically infeasible immediately after reunification.

To improve the efficiency of the local level, the state of Brandenburg first reformed the county level in 1992/1993. This reform had two central aspects. First, the number of counties was reduced to concentrate the higher local level administration. Second, an additional administrative local level was introduced (denoted the *Amt*), which would act in between small municipalities and the county. The administrations of several small municipalities were merged into an Amt. The purpose of an Amt was to provide the necessary resources that are required to perform all necessary administrative duties and to deliver economies of scale.[6]

Even after this initial reform in 1993, the local level was still facing three major difficulties. First, the Amt, which was introduced to capitalise on economies of scale, often failed to be able to coordinate the local activities. As municipalities were still formally independent, many administrative acts had to be performed individually for each municipality (see Grünewald, 2002). Second, the authorities within the Amt structure were often ill-defined, and conflicts arose among municipalities about issues such as the financing of the Amt. The leadership of the Amt often saw the need for specific policy interventions, yet they lacked the authority and the constitutional legitimacy.[7] Third, many of the small municipalities suffered from the lack of sufficient political competition. In 1998, a total of 152 communities did not hold the scheduled mayor election as no candidate put up for election.

4.2.2 The Reform Process 1999 - 2003

The debate about a second major reform of the local administrative structures, this time at the municipal level, began in 1998. In 2000, the state government of Brandenburg issued a decree of guidelines[8], which laid out basic criteria for the administrative structure that had to be satisfied in the future (see details below).

Municipalities were informed that they had the chance to merge voluntarily until the end of March 2002 to meet the targets of the guidelines. All municipalities that did not satisfy the guidelines in March 2002 would be merged by law at the discretion of the state government.[9]

[6]It is important to note that an Amt has no function or legislative power (other than coordination) in its own right. The municipalities remained the unit of political decision making. The Amt as the administrative unit only implemented and administrated the decisions.

[7]The faulty design of these local structures became apparent when the state constitutional court ruled that some Amt-structures were unconstitutional because the board of the Amt lacked democratic legitimacy (*Verfassungsgericht des Landes Brandenburg* 5/1995).

[8]The formal name is the 'Guidelines for the Development of Community Structures the State of Brandenburg' (*Leitlinien der Landesregierung für die Entwicklung der Gemeindestruktur im Land Brandenburg*) [LT-Drs. 21732-B.]

[9]Shortly after publication of the guidelines, the state legislature passed the 'Municipality Reform Act' (*Gemeindereformgesetz*). Purpose of the new law was to simplify voluntary mergers of municipalities by reducing legal requirements. According to the new law, there were three legal requirements for a merger to take place. Firstly, all municipalities involved in an amalgamation had to agree to a merger contract, which had to be enacted by the community council with an absolute majority of its members. Secondly, municipalities with less than 5,000 inhabitants had to hold public referendum on the proposed amalgamation. A positive referendum

While satisfying the criteria from the detailed guidelines was desired, the interior ministry also made it clear that exceptions were admissible under certain conditions.

The formal mergers took place between 2001 and 2003. The first wave of voluntary mergers happened already in February 2001, less than a year after the reform was announced. In March 2002, the interior ministry began drafting a law to implement mergers of those municipalities that had not agreed on a voluntary amalgamation. The law was enacted in March 2003 and the remaining mergers occurred in October 2003, the day of the local elections.[10]

Table 4.1 gives an overview of the timing of mergers and the number of municipalities involved in each merger. We say a merger happened voluntarily if the participating municipalities signed a contract. In contrast, forced amalgamations took place because of laws passed by the state legislature. The important difference between a voluntary and a forced amalgamation was that in a voluntary amalgamation, municipalities were free (within limits) to decide with whom to merge, and to agree on the terms and conditions of the merger. In total, we observe 349 mergers, which resulted in a loss of 1,067 municipalities, or roughly 72 percent (1,067/1,489) of the pre-reform number of municipalities. More than 70 percent (249/349) of all mergers occurred voluntarily, with the majority taking place between March 2001 (when the reform was launched) and October 2003 (when the last voluntary mergers were formed).[11] Comparing the size distribution of voluntary and forced mergers, we observe that voluntary mergers tend to be larger, with fewer mergers consisting of only 2 or 3 municipalities.

4.2.3 Guidelines

As mentioned above, even voluntary mergers had to be approved by the interior ministry, which evaluated whether or not the minimal requirements were met by the proposed merger. These guidelines formulated quantitative and qualitative goals. As a general rule, they stated that no additional administrative units were to be created, county district borders had to stay unaffected unless suburban municipalities were incorporated into cities with county district status. As a result, most mergers could take place only among municipalities within one county. Furthermore, mergers should take place within the borders of an existing Amt.

The guidelines moreover stated that municipalities that were independent of an Amt should

could prevent voluntary amalgamations by overruling the approval of the municipal council. Municipalities with more than 5,000 inhabitants were free to choose whether or not to hold a referendum. Finally, an amalgamation had to be approved by the interior ministry. A positive referendum could not overrule a negative decision by the ministry.

[10]Throughout the drafting process, municipalities could agree on voluntary mergers. If the mergers were admissible under the guidelines, they were approved and the draft of the law was changed accordingly.

[11]Note that there is a subtle difference in the group of voluntary mergers. Voluntary mergers generally happened before October 2003, and were enacted by individual merger contracts between the merging municipalities. However, a number of voluntary mergers also happened in October 2003 (at the same time as the forced mergers). These municipalities had agreed to merge voluntarily, but had passed the given deadline to write up a merger contract. As a result they were simply included into the merger law. We classify them as voluntary as they have chosen their partners themselves.

Table 4.1: Distribution of Municipality Mergers Across Different Reform Phases

	All Mergers 1999-2004		Voluntary Mergers before 2/2001		Voluntary Mergers from 3/2001 to 9/2003		Voluntary Mergers in 10/2003 (by law)		Forced Mergers in 10/2003	
	abs.	in %	abs.	in %	abs.	in %	abs.	in %	abs.	in %
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Number of Mergers	349	(100.0)	8	(100.0)	193	(100.0)	48	(100.0)	100	(100.0)
Merger size										
2	143	(41.0)	5	(62.5)	74	(38.3)	17	(35.4)	47	(47.0)
3	74	(21.2)	2	(25.0)	42	(21.8)	13	(27.1)	17	(17.0)
4	34	(9.7)	0	(0.0)	21	(10.9)	3	(6.3)	10	(10.0)
5	23	(6.6)	0	(0.0)	19	(9.8)	2	(4.2)	2	(2.0)
6	23	(6.6)	0	(0.0)	14	(7.3)	3	(6.3)	6	(6.0)
7-10	31	(8.9)	1	(12.5)	10	(5.2)	7	(14.5)	13	(13.0)
≥11	21	(6.0)	0	(0.0)	13	(6.7)	3	(6.3)	5	(5.0)
Municipalities lost	1,067		15		592		161		299	

Notes: Table shows the scope of the merger reform, distinguishing between different stages of the reform process. We present descriptive statistics on the distribution of merger sizes in each phase of the reform, reporting both the total number of mergers falling into a particular category, and the percentage share relative to the total number of mergers in a given phase.
Source: Statistical Office of Berlin-Brandenburg

be created only if there was a sufficiently high population density, and if a town existed that was sufficiently large to serve as social and economic centre of the new municipality. The newly created municipality should have at least 5,000 inhabitants, and if it was not possible to create independent municipalities of sufficient size, communities should merge within an Amt. An Amt should also have at least 5,000 inhabitants, and consist of no less than 3 and no more than 6 municipalities, with each participating municipality having at least 500 inhabitants. Furthermore, the travel distance from any municipality within an Amt to the seat of the administration should not exceed 20 kilometres.

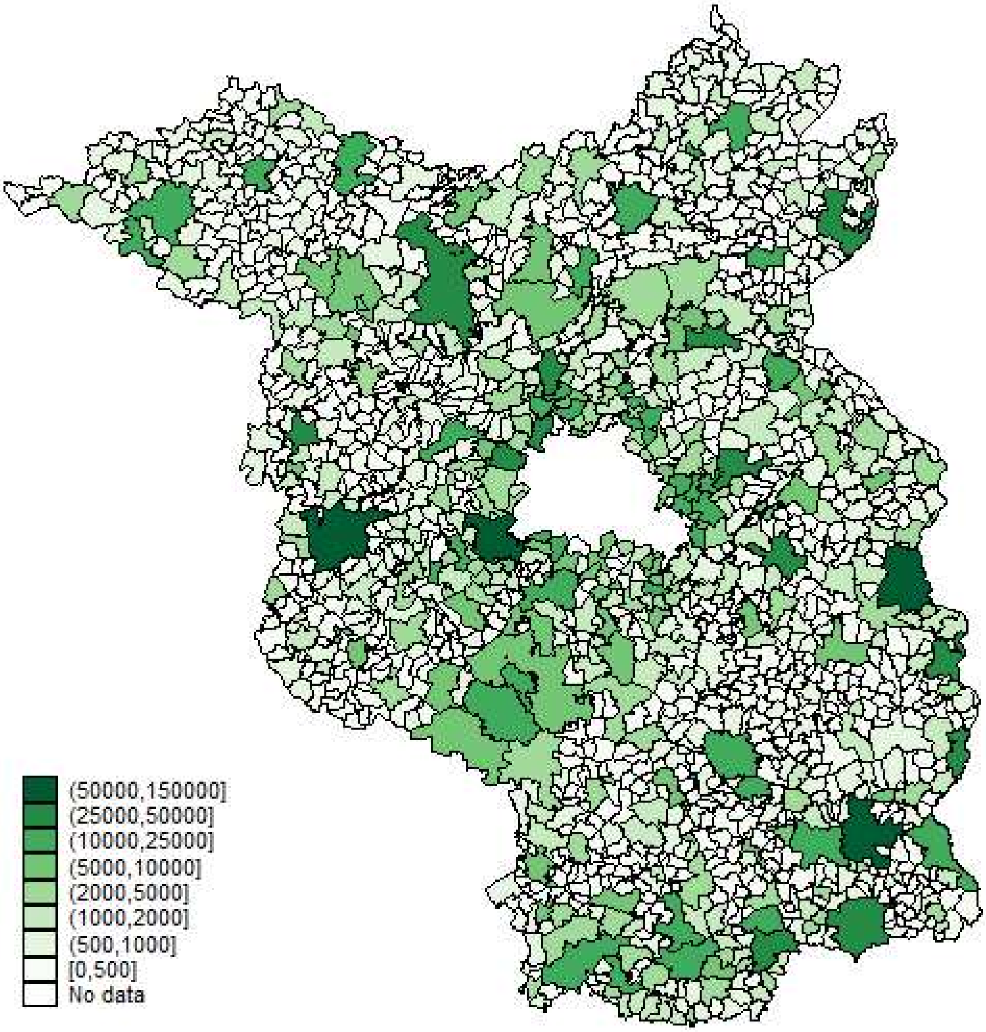
4.3 Data and Descriptive Statistics

We use data on all municipalities from the state of Brandenburg between 1999 and 2004. Brandenburg, one of the new states in the eastern part of Germany, is among the most rural and ethnically homogeneous states in Germany. As a preliminary, Figure 4.1 provides some graphical evidence on the spatial distribution of inhabitants across municipalities. The figure illustrates that larger municipalities cluster around Berlin, however, there is also quite a dispersion in municipality size throughout the state.

At the end of 1998, there were 1,489 municipalities in Brandenburg. Out of those, 874 had less than 500 inhabitants. To put this into perspective, by that time about one quarter of all German municipalities with less than 500 inhabitants were located in Brandenburg, although the state is home to only 3 percent of the German population. Out of the 1,489 municipalities, 1,485 belonged to 14 different counties.[12] Whereas 62 municipalities were

[12] The major cities of Potsdam, Frankfurt/Oder, Cottbus and Brandenburg city have the county status. Those

Figure 4.1: Spatial Distribution of Inhabitants per Municipality



Note: Figure shows the spatial distribution of inhabitants across municipalities in the state of Brandenburg. Darker areas indicate larger populations per municipality, and lighter colours indicate lower numbers of inhabitants.
Source: Statistical Office of Berlin-Brandenburg

independent of an Amt, a total of 1,423 municipalities belonged to an Amt structure.

For the analysis, we have compiled information on all 349 amalgamations that took place between 1999 and 2003, and involved a total of 1,276 municipalities (85.5 percent of all municipalities). For each merger, we know the municipalities concerned and we have information on the timing and the terms of the merger decision. A total of 249 amalgamations occurred voluntarily (by contract) whereas 100 were forced by law (see Table 4.1).[13]

For all municipalities we have geo-spatial data from the state's land survey office indicating

four cities are highlighted in dark green in Figure 4.1.

[13]It should be stressed that individual municipalities might have been affected more than once in the process of forming new mergers. In total there are 55 cases in which a community was involved in more than one amalgamation.

Table 4.2: Summary Statistics of Voluntary, Forced, and Simulated Mergers

	Voluntary		Forced		Simulated	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)	(5)	(6)
Number of observations	957		363		8,906	
Number of mergers	237		94		2,352	
Panel A. Municipality-level variables						
i. Demographics						
Population	1,319	3,853	1,894	3,626	1,225	2,747
Share of Population Aged 20-29	0.08	0.02	0.08	0.02	0.08	0.02
Share of Population Aged 30-45	0.22	0.03	0.22	0.03	0.22	0.03
Share of Population Aged 65+	0.23	0.05	0.22	0.04	0.23	0.05
Gender ratio (female/male) 20-29	0.91	0.53	0.89	0.42	0.89	0.45
ii. Expenditures p.c.						
Total	1,361.1	910.9	1,429.0	634.2	1,363.3	620.8
Schooling	43.1	65.5	59.9	91.8	48.1	71.8
Culture	28.9	96.5	20.1	43.7	24.2	77.7
Infrastructure	213.0	248.1	211.7	227.6	217.3	248.1
Public Utilities	94.9	185.2	80.5	120.5	93.0	171.4
HHI of all expenditure categ.	0.32	0.14	0.3	0.12	0.32	0.13
iii. Revenues p.c.						
Property Tax B (Devel. Land)	53.9	27.2	65.3	46.2	55.6	31.0
Property Tax A (Agric. Land)	17.1	14.6	11.9	11.0	15.8	14.0
Trade Tax	58.3	288.3	75.4	191.8	53.5	161.4
Interest Payments	29.1	47.8	24.6	45.3	28.5	46.3
Mun. Share of Income Tax	67.8	20.2	75.9	21.2	69.6	20.8
Mun. Share of VAT	12.6	60.6	18.4	36.3	12.5	27.4
Panel B. Coalition-level variables						
i. Demographics						
Total Population	7,786	15,187	9,548	6,871	5,704	8,689
(Max-Min) Population spread	5,456	14,174	6,143	5,427	2,524	3,303
Population diff. from coalition mean	4,622	13,301	4,860	4,925	1,778	2,709
Diff. Share of 20-29 from coalition mean	0.02	0.01	0.01	0.01	0.02	0.01
Diff. Share of 30-45 from coalition mean	0.02	0.02	0.03	0.03	0.03	0.02
Diff. Share of 65+ from coalition mean	0.04	0.03	0.04	0.03	0.04	0.03
Diff. female share among 20-29	0.35	0.46	0.29	0.35	0.33	0.41
Mergersize (no. of municipalities)	6.31	4.33	5.86	3.59	5.52	3.36
Share of dominant party	0.33	0.28	0.27	0.24	0.34	0.28
ii. Expenditures						
Total	607.2	995.8	521.7	506.2	550.9	530.4
Schooling	54.7	67.8	76.8	88.9	55.2	74.2
Culture	47.4	105.4	25.7	37.9	34.9	86.7
Infrastructure	194.0	231.5	155.2	197.3	186.7	222.7
(Max-Min) Total	1,322.8	1,259.1	1,167.8	908.3	1,139.3	821.1
(Max-Min) HHI	0.28	0.17	0.23	0.16	0.25	0.17
iii. Revenues						
Property Tax B (Devel. Land)	43.5	46.8	63.9	74.6	41.9	46.8
Property Tax A (Agric. Land)	21.2	15.9	17.8	12.0	20.0	17.0
Trade Tax	153.5	412.6	186.5	336.1	142.6	288.2
Interest Payments	80.0	76.5	61.1	66.7	67.5	67.0

Notes: Table reports summary statistics on key variables in our analysis sample. Each municipality is weighted equally.
Source: Statistical Office of Berlin-Brandenburg

the location of the municipality as well as county, Amt and municipality borders. Information on elections is available from the state's election office. For 1,474 municipalities we know the composition of the city council at the time of a merger. We also have data on the party affiliation of the mayor for 1,228 municipalities.[14]

We have access to data about important financial (revenues as well as expenditures) as well as socio-economic characteristics for each municipality. The socio-economic variables include the total population, the share of young, middle-aged and old citizen as well as the gender ratio (female/male) in the young cohorts. The data on revenues include tax rates and tax revenues of those taxes that municipalities can levy themselves as well as the municipalities' share of federal taxes. The municipalities' share of the federal income tax is directly computed from the total income tax raised within each municipality and can therefore serve as a proxy of wages earned within the municipality. Similarly, revenues from the real estate tax serve as a proxy for real estate prices within the municipality.[15] In addition to the revenue side, we have also obtained data on the expenditures of municipalities, which provides us with a detailed overview of local preferences in terms of public spending. In particular, we have, in addition to total expenditures, information on the subcategories schooling, culture, infrastructure, and public utilities.

In order to obtain representative figures for the financial structure, we average the revenue and expenditure variables over the years 1999 and 2000. That is, we only use data from the period *before* any plans of the reform became public, and therefore avoid that our estimates are influenced by spending that was done in anticipation of the reform.[16] Also, using the 1999 and 2000 data has the added advantage that it allows us to control for spending preferences of the *current* local governments, elected in 1998.

As the main analysis uses a comparison of voluntary to forced mergers as well as voluntary to simulated mergers, we provide descriptive statistics on all variables used in the estimation separately for those groups in Table 4.2. What stands out from this table is that the groups are quite different. Forced mergers are larger on average, involve fewer municipalities and show lower average seat shares of the dominant party than voluntary mergers (see Table 4.2, coalition-level variables).[17] For the simulated mergers, we see that the average number of

[14]The discrepancy to the total number of municipalities is due to several reasons. As noted above, there are 152 cases in which municipalities did not have elections for the post of mayor due to a lack of candidates. In those cases, the community council elected a mayor but he or she did not necessarily belong to a political group, or had no recorded party affiliation. 28 municipalities had less than 100 inhabitants and elected a mayor in a public village meeting. Again there is no party affiliation recorded in those cases. In 35 cases the election was invalid because no candidate received an absolute majority in the run-off elections or the number of votes for the leading candidate was less than 15% of eligible voters. A mayor was then elected by the municipality council and no party affiliation recorded. In 4 cases no party affiliation of the mayor was recorded and we were unable to obtain it otherwise. Finally, there were no elections held in 42 municipalities or the election results were not recorded for reasons unknown to us.

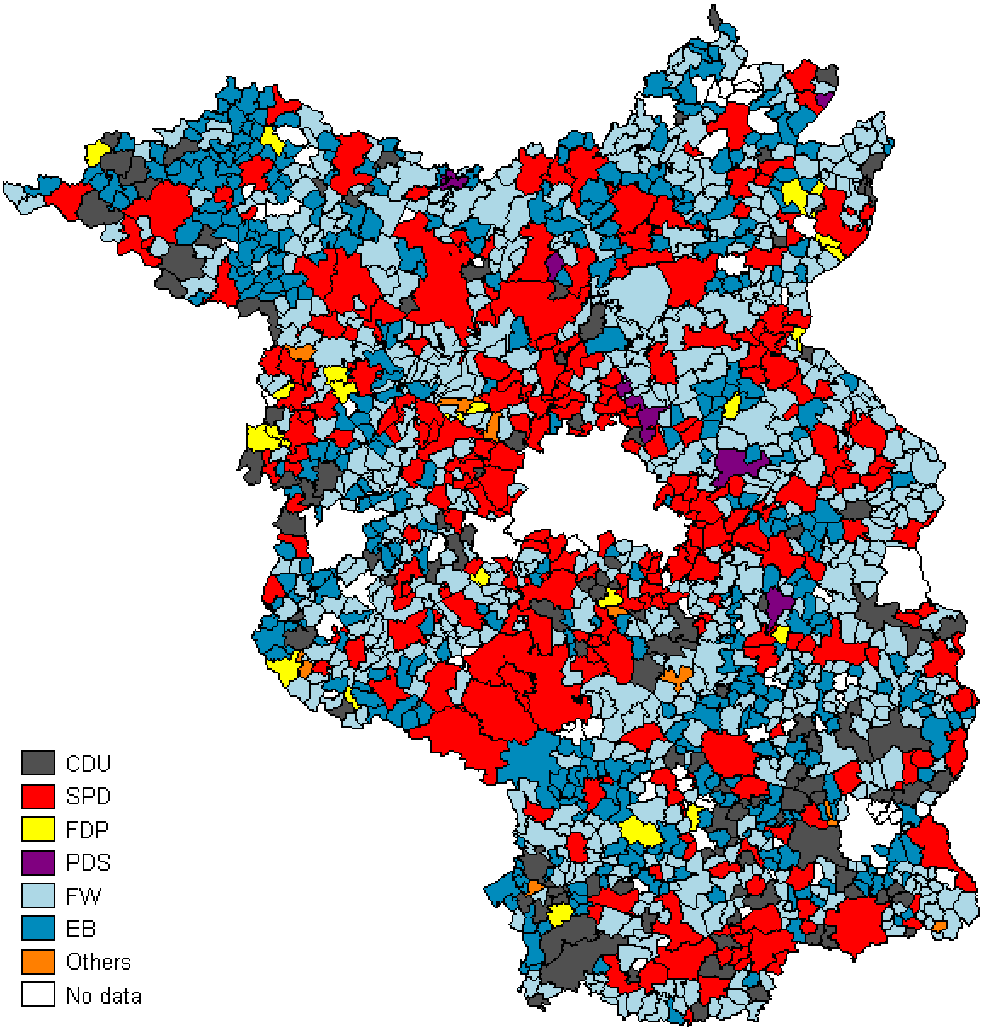
[15]It should be noted, however, that in the case of real estate, book values and actual values of property might differ significantly, and therefore real estate tax revenue is only an imperfect proxy for actual land and housing prices.

[16]As shown by Tyrefors Hinnerich (2009) this is a concern because municipalities tend to free-ride on each other in anticipation of mergers by increasing their spending in the years just before the merger takes place.

[17]Overall, those differences are expected. The forced mergers in the second stage are conditional on the mergers in the first stage of the reform. As a result, the municipalities are large on average. Also, the political

inhabitants is in fact smaller.[18] A nice feature of the data is that voluntary and simulated observations have comparable distributions in the political variable of interest.

Figure 4.2: Spatial Distribution of the Dominant Party per Municipality



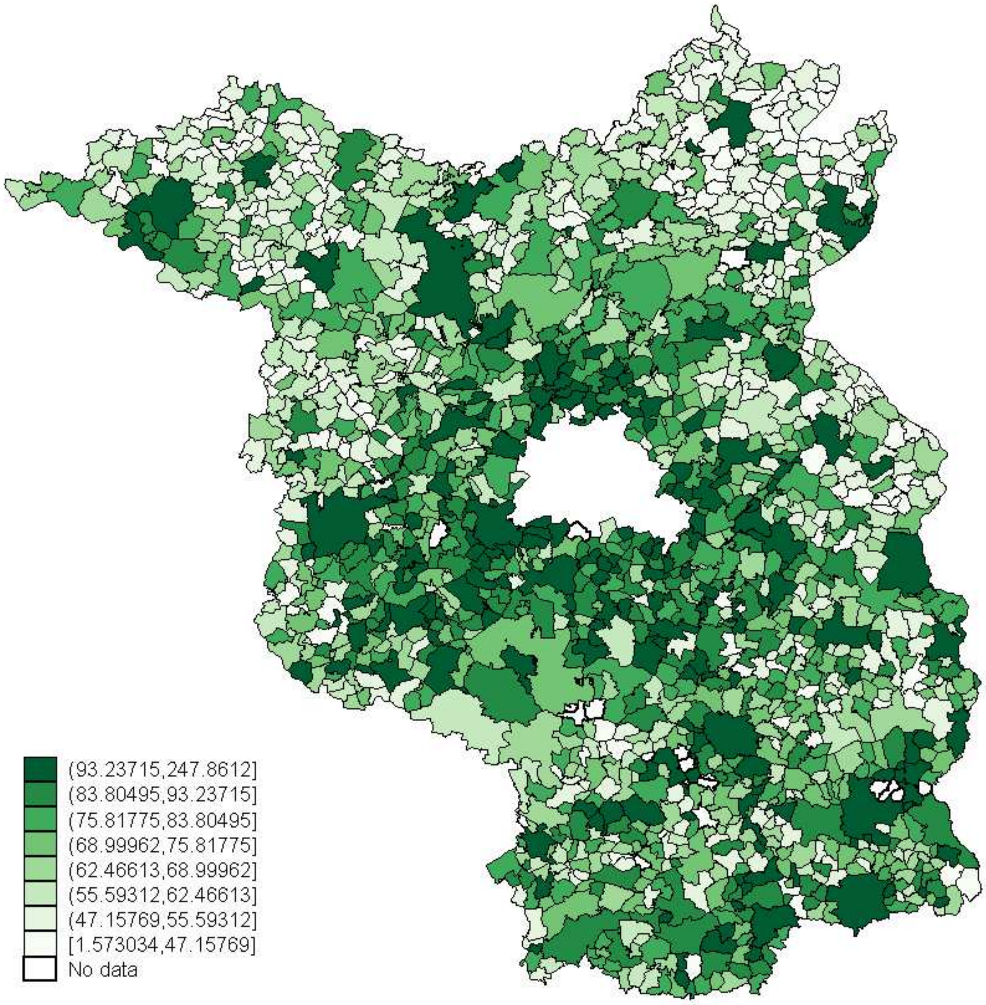
Note: Figure shows the spatial distribution of the dominant party in the community council across municipalities in the state of Brandenburg. Each party is encoded by an individual colour.
Source: Statistical Office of Berlin-Brandenburg

As mentioned above, one obvious concern when analysing merger decisions is a potential bias due to spatial correlation. Such a bias might arise if certain municipality characteristics were correlated across space. Indeed, looking at the spatial distribution of our political variable (Figure 4.2) and the municipality's share of income tax revenues as a proxy for local

landscape in large towns is often more heterogeneous and knows a larger number of parties.

[18]There are two main arguments for this observation. First, municipalities that considered merging voluntarily might have had a preference for larger partners. Second, the set of simulated mergers that we constructed includes mergers with small merger partners that the state agency would not have authorised. In a robustness test, we exclude those particular mergers.

Figure 4.3: Spatial Distribution of the Municipality Share in Income Taxes



Note: Figure shows the spatial distribution of the share of income tax revenues at the municipality level for the state of Brandenburg. Tax revenues are divided by the total number of inhabitants. Darker areas indicate higher tax revenues per inhabitant, and lighter areas indicate lower revenues.
Source: Statistical Office of Berlin-Brandenburg

income (Figure 4.3), we observe a clear clustering in those variables. Though suggestive at most, the patterns indicate that municipalities located around Berlin (in the centre of the state) have higher expenditures and revenues, and tend to be run by the social democratic party (SPD). Let us emphasise again that this spatial correlation is *a priori* not a concern for our empirical approach, as it prevails in both the voluntary and the counterfactual (forced or simulated) observations. Nevertheless, to ensure that our estimates are not driven by spatial correlation in pre-existing municipality characteristics that potentially alter the merger decision, we include in our models a variety of pre-reform controls that capture local spending and political preferences as well as the socio-demographic structure.

4.4 Empirical Strategy

The main statistical model that we use in this application is a standard Probit model. We adopt two estimation designs: our first design exploits the particular structure of the reform by contrasting voluntary and forced mergers. Our second design compares voluntary mergers to a large set of simulated mergers. We describe the exact implementation of each of the designs in the next two subsections.

4.4.1 Voluntary vs. Forced

Our central analysis is based on a standard Probit model in which we code the decision to join a voluntary merger (treatment) by 1, and use forced mergers, coded as 0, as counterfactual observations. Specifically, the model takes the following form:

$$Pr(Y_i = 1|X) = \Phi(\beta_0 + \beta_1 * Pol + X_i\gamma) \tag{4.1}$$

where *Pr* denotes a probability and Φ is the CDF of the standard normal distribution. The unit of observation is the individual municipality within a merger with its characteristics before the merger. This implies that there are as many observations for one merger as there are merger partners. Note, that one municipality can only be part in one merger at a time, but may partake in another merger at a later stage.[19]

Our focus in the analysis is to evaluate the importance of political determinants for the likelihood of a voluntary merger. As the municipality council is the decision body, we will focus on political circumstances within this legislative institution. More specifically, we will concentrate on the dominant party within the council, assuming that this party has the most voting power in the merging decision. Our central variable in the analysis, *Pol*, is defined in two steps. In the first step, we identify the dominant party in each town council.[20] In the second step, we calculate the population weighted share of that party within the town councils of the *partners* in the merger, i.e., excluding the municipality under consideration. We consider this variable as a measure of political congruence. We conjecture that a higher share should increase the likelihood of a voluntary merger if local politicians prefer to merge with other municipalities that are also dominated by their party.

As mentioned above, one may be concerned about omitted variable bias. In particular, the political variables might proxy for important unobserved determinants correlated with the merger formation. For example, while the observation of two municipalities under conservative rule deciding to merge voluntarily might be due to political congruence, it is

[19]Subsequent mergers may occur within the voluntary phase and/or the phase when mergers were enforced by state. Clearly, multiple mergers are less likely since every single merger had to be ratified by the state authority, which ensured that every single merger satisfied the guidelines. In the rare event that a municipality is first part of one merger during the early phase of the reform, and the new (bigger) municipality participates again in another merger at a later point in time, we treat the merged municipality in the second merger as an individual municipality.

[20]As the mayor of the town also holds one seat in the council, we added the information of the local mayor's party affiliation to the data on the party's council seats.

also possible that political congruence merely proxies for the preferences of high earning voters that prefer both conservative rule and a merger with another equally rich municipality. In order to deal with this potential problem, we have collected an extensive set of control variables that cover most factors correlated with political preferences on the local level. In X in eq. (4.1), we include up to 40 control variables which can be grouped into five categories. We include regional dummies, results from elections other than for the community council, demographic characteristics of inhabitants, as well as expenditures and revenues of municipalities by category. The information on the fiscal status of each municipality comprises the entire local budgetary information. Hence, we are no less well informed about the financial situation than potential merging partner were at the time. Especially the tax revenue data can be considered a perfect proxy for income or wealth in the town, since these taxes are based on personal income or property prices. An overview of all control variables is provided in Table C.1 in the Appendix.

4.4.2 Voluntary vs. Simulated Mergers

In our second design, we use a comparable statistical approach based on the following Probit specification:

$$Pr(Z_i = 1|X) = \Phi(\beta_0 + \beta_1 * Pol + X_i\gamma) \tag{4.2}$$

The main difference to eq. (4.1) above is the new outcome variable, Z_i. The variable Z_i is again an indicator variable equal to 1 for observations in voluntary mergers. However, the new comparison group now consists of simulated mergers. Observations from these simulated observations are coded as 0 for the outcome variable.

The remainder of the model is identical to the model above. In fact, we particularly aim to keep the model specifications in the two designs as similar as possible. That is, we calculate the same political congruence variable for the simulated mergers, and also define all control variables accordingly.

To better understand the value of this second design, it is important to know how we simulate our counterfactual observations. We start the simulation using all municipalities as of 1999 (two years before the first actual mergers).[21] The simulation proceeds in three steps. First, we randomly pick one municipality, identify all direct neighbours (in the same county) of this municipality and randomly assign the first merger partner. With probability λ_1, we stop at this stage in which case we have a simulated merger size of two. With probability $(1-\lambda_1)$ we proceed to step two. Here, we first flip a coin between the two municipalities and then again identify the set of direct neighbours (in the same county) for the chosen municipality and pick a third merger partner. At this stage, we test this new merger for plausibility. We do not allow units that belong to a specific Amt administration to be united

[21] In an additional robustness check, we also ran the simulation such that only municipalities that actually merged (voluntary or forced) were used as a starting point of the simulation procedure.

with partners from outside this Amt structure. With probability λ_2 we stop at this stage (we then have a merger of size three). In the third step, we iterate on the procedure in step two and use the algorithm to choose more merger partners as we proceed. Importantly, the distribution of λ's is set such as to match the actual distribution of merger size for the observations of voluntary mergers.[22] With this algorithm, we can then simulate an arbitrary number of counterfactual observations. We use 10 times the number of actual voluntary mergers, i.e. about 2,500 simulated mergers.

4.4.3 Issues in Estimating Merger Determinants

Estimating the determinants of mergers is statistically challenging. The specific problems for the estimation have been characterised by Gordon and Knight (2009) for the case of school district consolidations, and for municipal mergers for Tukiainen et al. (2013). Our description of the econometric issues largely follows theirs.

First, in voluntary mergers, the joint decision to form a new unit depends on a positive decision of each of the members. The problem is therefore multi-sided. A merger that actually occurred, only happened because all members in the merger agreed to join this union. In comparison, it is enough that one member refuses to join a potential merger for this alternative merger not to realise, even if all other members were favourable towards this union.

This implies that the best data to study the determinants of merger decisions would consist of information on individual decisions to join or not to join certain merger alternatives. We, however, only observe mergers that actually realised and those that did not. In particular, we do not know whether a certain municipality favoured a particular merger alternative, but it was blocked by other members.[23]

Second, a further and related issue is that municipal mergers, like in many coalition formation games, are one-to-many matches. In our example, we observe a whole distribution of merger sizes ranging from 2 members all the way up to 19 members. When constructing simulated mergers, this means that the dimensionality of the potential mergers is largely increased. This raises the question how the *relevant* potential mergers can be picked from the set of potential mergers.

Finally, the analysis suffers from the fact that a merger decision of one group has a direct impact on the decisions of neighbouring municipalities. Once a group of municipalities

[22]Note that the chances of of picking a particular simulated merger that represents an actual voluntary merger is small. Nevertheless we test whether the set of simulated (counterfactual) observations includes mergers which actually happened. In this case, we exclude those cases from the analysis.

[23]To remedy this problem, we experimented with a bivariate Probit Poirier model as suggested by Brasington (2003). The idea is to independently model the decision of the individual member to join a certain club and the decision of the group to take in that member in a bivariate Probit model. This type of statistical modelling requires strong arguments to exclude variables in one of the equations. In essence, such a model only improves our understanding of the merger decision if we can assume that certain factors serve as strong predictors only on the side of the individual member or the receiving group. While the model can still be applied even without such strong exclusions, we experience issues of convergence of the estimator, a problem also reported by Saarimaa and Tukiainen (2014).

decides to merge, these towns are eliminated from the choice set of the adjacent municipalities (a violation of the stable unit treatment value assumption (SUTVA)). Unfortunately, we do not have a way to avoid this problem for our estimation.[24]

4.5 Results

In this section, we present our results. We proceed in three steps. We begin our discussion with the comparison of voluntary and forced mergers; then we turn to the results from contrasting voluntary to simulated mergers. In the last part of our analysis, we report a number of robustness checks to confirm our main conclusions.

4.5.1 Voluntary vs. Forced Mergers

Table 4.3 highlights the results from the Probit model in eq. (4.1). The dependent variable is an indicator variable equal to one if the municipality took part in a voluntary merger, and zero if it was placed in a forced amalgamation. The variable of interest is the seat share of the municipality's dominant party in the town councils of the merging partners (weighted by population) which is a scale variable between 0 and 1. A positive coefficient indicates that a higher share of the dominant party in the potential coalition increases the probability to observe an individual municipality in a voluntary merger. We report coefficients and standard errors (clustered at the merger level), and the marginal effects from the Probit estimation (shown in square brackets).

Column 1 presents the results of the most parsimonious model which only takes the political variable as an explanatory factor. Our variable that measures the importance of political congruence is positive and statistically significant. The marginal effect of 0.191 (evaluated at means) can be interpreted as follows. Say the town council of municipality A is led by the conservative party (CDU). Let the hypothetical situation be that municipality A can choose to join either a coalition of potential partners in which the conservative party holds no seats at all versus a coalition in which the conservative party holds the entire council with 100% representation.[25] The marginal effect indicates that municipality A is 19,1 % percentage point more likely to join the second coalition voluntarily, which is a sizeable effect. The explanatory power of this simplistic model, however, is very limited (as indicated by the pseudo R-squared).

In columns 2 through 6, we gradually test different sets of further control variables, all of which introduce meaningful further variables that should have an impact on the decision

[24]Saarimaa and Tukiainen (2014) run a robustness test in which they use as counterfactual simulated control group only potential mergers that did not involve any municipality that was actually merged. While this is a nice idea, it is also easily done in the Finnish case, which only involved relatively few merged municipalities to start with. In our example, however, the merger reform affected more than 90% of all municipalities, which leaves the sample of never treated municipalities to be very small. Importantly for us, Saarimaa and Tukiainen (2014) report that the results from this robustness test left their results relatively unchanged.

[25]Note that this is not all that unlikely in small communities. However, for the most part the variable of interest does not vary sharply from 0 to 1.

Table 4.3: Probit Regression Results: Voluntary vs. Forced

	Basic specifi- cation (1)	(1) + Region (2)	(2) + Political (3)	(3) + Demo- graphic (4)	(4) + Major Budget (5)	(5) + Detailed Budget (6)
Dominant Party Share	0.5743	0.6640	0.5978	0.5624	0.5084	0.3943
(within coalition partners)	(0.2288)	(0.2451)	(0.2491)	(0.2473)	(0.2463)	(0.2428)
	[0.1908]	[0.2118*]	[0.1888]	[0.1748]	[0.1564]	[0.1169]
Selected controls						
Community size				-0.0000	-0.0000	-0.0000
				(0.0000)	(0.0000)	(0.0000)
Δ Community size				0.0002	0.0002	0.0002
				(0.0000)	(0.0001)	(0.0001)
Total Pop. Involved				-0.0002	-0.0001	-0.0001
				(0.0000)	(0.0000)	(0.0000)
Merger Size				0.0839	0.0820	0.0885
				(0.0376)	(0.0436)	(0.0432)
Total Expenditures					-0.0001	-0.0002
					(0.0001)	(0.0001)
Rev. Property Tax A					0.0077	0.0056
					(0.0053)	(0.0051)
Rev. Property Tax B					-0.0009	-0.0006
					(0.0013)	(0.0013)
Rev. Trade Tax					0.0002	0.0006
					(0.0002)	(0.0004)
Rev. Interest payments					0.0003	0.0009
					(0.0009)	(0.0010)
(Max-Min) Total Expend.					-0.0000	-0.0001
					(0.0002)	(0.0002)
(Max-Min) Rev. Prop. Tax A					0.0002	-0.0007
					(0.0077)	(0.0074)
(Max-Min) Rev. Prop. Tax B					-0.0038*	-0.0034
					(0.0020)	(0.0020)
(Max-Min) Trade Tax					0.0004	0.0007
					(0.0003)	(0.0003)
(Max-Min) Rev. Int. payments					0.0023	0.0025
					(0.0017)	(0.0018)
Fixed Effects						
Regional Dummies		✓	✓	✓	✓	✓
Political Controls			✓	✓	✓	✓
Demographics				✓	✓	✓
Budget Composition						✓
Observations	1,320	1,320	1,317	1,314	1,314	1,314
Pseudo R-squared	0.0104	0.0994	0.1170	0.1792	0.2015	0.2289

Notes: Table shows coefficient estimates of Probit regressions of an indicator of voluntary mergers on the dominant part share in a coalition. A leading Δ indicates variables measured as differences to the coalition mean. The regression constant is not reported. Max-Min spreads are computed at the coalition level. Fiscal and financial variables are measured in per capita values. Standard errors are reported in round parentheses and marginal effects at the mean in square brackets. All standard errors are clustered on the merger level.
Source: Statistical Office of Berlin-Brandenburg

to amalgamate. To control for possible coordination between municipalities and parties at the county level, we include, in column 2, a complete set of regional dummies (there are 14 counties in the state of Brandenburg). Doing so, ensures that our results are not driven by differences between these regional entities. Column 3 presents the results when we include further political variables. We control for both the party identity of the mayor and for heterogeneous effects of which specific party dominates the town council. Furthermore, we test whether the party identity of the mayors in the merging partners of the coalition is relevant.[26] Overall, the inclusion of these controls does not alter the impact of political congruence.

Column 4 includes variables on the demographic structure in the municipalities into the model. We add municipality size, total number of inhabitants in the new merger, the difference between the own population and the mean population in the new merger. We also include measures on the age structure (share young, share old) as well as a gender ratio variable (not reported). In the next columns, we additionally control for important budgetary information. It is important to remember that we have full information on all budgetary items that the state requires the municipalities to report. We are therefore in the position to understand the financial situation of a given municipality just as well as any potential merger partner at the time of the merger decision. In column 5, we first add measures for the aggregate budget (total expenditures, total revenues and revenues from the major taxes), and in column 6, we further include detailed subcategories of the expenditures (for the full list of variables, see Table C.1 in the Appendix) to measure the local preferences for spending. Each expenditure variable is included in levels and in terms of the Max-Min deviation within the coalition.[27]

Overall, our results for the variable of interest remain rather stable at around 17-21 percentage points in the marginal effect throughout specifications 1-4. However, in columns 5 and 6, the point estimates and significance levels drop substantially, down to insignificant 11.7 percentage points in the marginal effect when detailed financial information are controlled for. Still, even if the estimates are marginally not significant in column 6, the magnitude of the point estimate remains sizeable. In terms of standard deviation changes, a marginal effect of 11.7 corresponds to about one quarter of a standard deviation on the outcome variable. Our conclusion from this first part of the analysis is that political congruence has a sizeable and important influence on the merger decision. Yet, the results further highlight the importance of controlling for the financial background information of the participating municipalities.

[26] In an alternative model, one could argue, that the mayor who administrates the municipality might herself have stakes in the merger decision and has incentives to push amalgamations with other municipalities that have mayors of the same party. We do in fact, find only weak evidence of this hypothesis. Here, however, we focus on the effect of congruence within the town councils, because they formally are the legislative body deciding on the actual mergers.

[27] The Max-Min deviation measures the deviation in a fiscal measure from the minimum value (one merger partner) to the maximum value (another coalition partner), following the approach of Saarimaa and Tukiainen (2014). The focus, here, is on the largest distance of the most extreme values (minimum and maximum) within the merger partners. This measure takes into account that a coalition can only form if all partners agree to the merger. To that extent, it is important that each municipality in the merger can agree to be partner with the most extreme partners. We use this measure instead of the absolute deviation of a municipality's value to the population weighted mean. However, we use that alternative specification in a robustness test.

4.5.2 Voluntary vs. Simulated Mergers

In Table 4.4, we turn to our second design, the comparison of voluntary versus simulated mergers. The dependent variable in this model is equal to 1 for observations of municipalities in voluntary mergers and 0 for observations that belong to the set of simulated mergers. The table follows the same structure as Table 4.3.

In columns 1 to 3, the estimates of the reduced specifications are small and insignificant. However, when also controlling for detailed demographic variables (column 4) the estimates turn significantly and sizeable positive. The results, then, remain stable when we add detailed financial information to the model in columns 5 and 6.

Focusing on the full model (column 6), the coefficient estimates are around 24.1 (12.3), corresponding to about 3.45 percentage points in the marginal effects (evaluated at means), which again constitutes a considerable and significant effect. It is important to note that this effect is qualitatively consistent with our results from design 1. To see this, note that in the comparison of voluntary to simulated mergers, we observe 10 times as many simulated as voluntary mergers. In terms of standard deviations, the above effect constitutes roughly one ninth of a standard deviation difference in the outcome variable. While this is obviously still smaller than the effect in our first design (one quarter), it remains in an (economically) relevant magnitude. Taken together, out interpretation of these results is that political congruence indeed constitutes an important determinant in the merger process.[28]

4.5.3 Robustness

We next turn to probe the robustness of our results against various alternative specifications and variable definitions. First, we experiment with alternative ways to proxy for financial heterogeneity between merger partners and test a specification in which we drop the detailed subcategories of expenditures from the model, and instead use a summary statistic. For this measure, we calculated the Hirschman-Herfindahl Index (HHI) for the subcategories of spending as shares of the total budget (see Table 4.2 for summary statistics). The idea of this measure is that highly concentrated towns (high HHI) show particular preferences for spending. We, then, also calculate the difference between the individual HHI and the group mean (within the merger partner), which we consider to proxy for similarities in spending preferences. We present the results of the modified specifications in columns 1 and 2 in Tables C.2 and C.3 of the Appendix for the comparison of voluntary to forced and voluntary to simulated mergers, respectively. In all models, we find significant and positive estimates which are similar to our baseline results.

In our main specifications, we decided to proxy for heterogeneity in core budgetary items by including the Max-Min spread within the merger (see above). However, as this presents only one potential way of controlling for such heterogeneity, we additionally test specifications in which we use the difference between the individual municipality and the

[28]It is also reasonable that the level of significance is higher in the comparison of voluntary versus simulated. There are almost 30 times more simulated observations as we have actual forced merger observations.

Table 4.4: Probit Regression Results: Voluntary vs. Simulated

	Basic specification (1)	(1) + Region (2)	(2) + Political (3)	(3) + Demographic (4)	(4) + Major Budget (5)	(5) + Detailed Budget (6)
Dominant Party Share	-0.0204	-0.0206	-0.0485	0.2674	0.2508	0.2407
(within coalition partners)	(0.1078)	(0.1062)	(0.1164)	(0.1227)	(0.1240)	(0.1227)
	[-0.0035]	[-0.0035]	[-0.0082]	[0.0392]	[0.0361]	[0.0345]
Selected controls						
Community size				-0.0000	-0.0000	-0.0000
				(0.0000)	(0.0000)	(0.0000)
Δ Community size				0.0002	0.0002	0.0002
				(0.0000)	(0.0000)	(0.0000)
Total Pop. Involved				-0.0001	-0.0001	-0.0001
				(0.0000)	(0.0000)	(0.0000)
Merger Size				0.0694	0.0789	0.0779
				(0.0214)	(0.0263)	(0.0266)
Total Expenditures					-0.0001	-0.0001
					(0.0000)	(0.0001)
Rev. Property Tax A					0.0059	0.0052
					(0.0021)	(0.0024)
Rev. Property Tax B					-0.0009	-0.0007
					(0.0007)	(0.0007)
Rev. Trade Tax					0.0002	0.0003
					(0.0001)	(0.0001)
Rev. Interest payments					0.0003	0.0004
					(0.0005)	(0.0005)
(Max-Min) Total Expend.					0.0000	0.0000
					(0.0001)	(0.0001)
(Max-Min) Rev. Prop. Tax A					-0.0084	-0.0081
					(0.0040)	(0.0041)
(Max-Min) Rev. Prop. Tax B					0.0002	0.0002
					(0.0012)	(0.0012)
(Max-Min) Trade Tax					-0.0000	-0.0000
					(0.0002)	(0.0002)
(Max-Min) Rev. Int. payments					0.0010	0.0010
					(0.0008)	(0.0008)
Fixed Effects						
Regional Dummies		✓	✓	✓	✓	✓
Political Controls			✓	✓	✓	✓
Demographics				✓	✓	✓
Budget Composition						✓
Observations	9,863	9,863	9,861	9,825	9,825	9,825
Pseudo R-squared	0.0000	0.0052	0.0076	0.0669	0.0744	0.0770

Notes: Table shows coefficient estimates of Probit regressions of an indicator of voluntary mergers on the dominant part share in a coalition. A leading Δ indicates variables measured as differences to the coalition mean. The regression constant is not reported. Max-Min spreads are computed at the coalition level. Fiscal and financial variables are measured in per capita values. Standard errors are reported in round parentheses and marginal effects at the mean in square brackets. All standard errors are clustered on the merger level.
Source: Statistical Office of Berlin-Brandenburg

group mean (within the merger partners). We show the results of those models in columns 3 and 4 of Tables C.2 and C.3 in the Appendix. The results for our variable of interest are of similar size and remain significant for the specification with simulated and voluntary mergers (see Table C.3). Additionally, we also run a placebo test in which we compare forced to simulated mergers (results not reported). As expected, we find no effects of political congruence.

Finally, we tested our results against alternative specifications related to the choice of the model, the sensitivity to excluding particularly large mergers, and changes in the set of simulated mergers. Table C.4 displays the results for these further robustness tests. The first two columns present the results of using Logit instead of Probit models for the contrasts between voluntary vs. forced and voluntary vs. simulated, respectively. The estimated effects are very close to our preferred Probit specifications. Columns 3 and 4 show the results when excluding large mergers (11 or more merging partners). These mergers are relatively rare events, however, they account for a substantial amount of data in our analysis because each municipality is included individually. The results are indeed interesting: while the size of the effects remain stable, we lose significance for the comparison of voluntary versus simulated, but gain significance in the specification using voluntary versus forced merger. The model in column 5 uses a smaller set of simulated mergers. Here, the simulation starts from a subset of communities that were actually involved in at least a single merger between 1999 and 2004. We find slightly smaller point estimates and lose significance. Finally, the model in column 6 also restricts the set of simulated mergers to those that involve a total population of at least 1,000 and no more than 50,000 inhabitants. We do this because in the simulation procedure, we are likely to identify small mergers that would not have been granted permission by the state authorities. In this specification, the estimated effects increase slightly and retain a similar significance level as in the main specification.

4.6 Conclusions

This paper studies the political determinants of municipality amalgamations. We estimate the effects of political congruence between potential partners on the probability of forming a voluntary merger during a boundary reform in the German state of Brandenburg, which substantially reduced the number of municipalities from 1,489 to 421 communities. To conduct the analysis, we constructed a data set comprising of detailed fiscal and socio-economic information on all municipalities in the state of Brandenburg through the period of the reform between 1999 and 2004.

To identify the effect, we follow a dual approach. First, we compare voluntary to forced mergers. The forced mergers happened in the second phase of the reform and merger partners were assigned by state authorities. Thus, we can assume that local political considerations did not matter in this stage of the boundary reform. Second, we analyse voluntary mergers relative to a set of simulated mergers. Here, we programmed a simulation algorithm that randomly assigns merger partners conditional on a set of parameters (following the guidelines of the actual reform). Also, in those simulated mergers, political congruence is not a decisive parameter, hence, we can use these observations as counterfactuals.

We find that political variables had a sizeable and significant effect on the decision to form a merger during the voluntary stage of the reform. Importantly, our two different designs yield largely similar results. This is particularly reassuring for the main argument of the paper, as the two designs have quite opposite strength and weaknesses when it comes to identifying our main effect.

Our results indicate that it is important to carefully consider the role of political decision makers in such reforms. As of today, the relevant literature often assumes that politicians act in the best interest of the municipality, or simply represent the median voter's position. Understanding the incentive structure of the political actors is even more important as the objective function of those decision makers might differ (perhaps strongly) from the initial intentions of the reform.

A Appendix to Chapter 2: Assessing the Role of Workplace Heterogeneity in Recent Trends of the Gender Wage Gap

A.1 Data Set and Additional Summary Statistics

A.1.1 The LIAB Mover Model 9308

My analysis uses the LIAB Mover Model 9308, a linked employer-employee data set prepared and provided by the German Institute for Employment Research (IAB). This data set links three main data sources: 1. Integrated Employment Biographies (IEB) with worker (spell) data, 2. Establishment History Panel (EHP) with administrative data on establishments, and 3. IAB Establishment Panel (EP) with survey data on establishments. The three data sources can be matched via three identifiers: a unique person-identifier (PID) and two establishment identifiers (EID1, EID2) that are all stored in the IEB file. In the following, I label the identifier for the EHP data as EID1, and the identifier for the EP as EID2.

The mover model (MM) belongs to the longitudinal versions of the linked employer-employee data sets, including the complete employment biographies of all workers selected into the sample. It differs from earlier versions of the longitudinal models (LM1-3) as well as the current version (LM) mainly in terms of the sampling design (see also Heining et al., 2012, 2014). The construction starts with all workers who are employed at two different establishments from the EP as of June 30th in two different (not necessarily consecutive) years. In practice, one could imagine the sampling procedure in two steps. First, all workers with at least one spell with non-missing EID2 are selected (previously restricting spells to main employment and covering June 30th). From this sample, only those workers are retained, whose employment biographies contain at least two distinct EID2's. For these EID2's, it must be ensured that the establishment departed from has a valid interview in the year of departure, and that the establishment moved to has a valid interview in the year of arrival. After eliminating recalls (dupicate PID-EID2 observations), this gives a sample of unique PID-EID2 combinations. For each (unique) EID2 in this sample, up to 500 additional employment biographies who are also linked to that firm are then randomly sampled and added to the data set. If an establishment employs less than 500 different workers between 1993 and 2008, all workers are sampled. These 'additional' workers either do not move or move to an establishment not participating in the EP. The total number of observed individuals at an establishment finally consists of the randomly sampled individuals plus all movers.

The sampling design of the mover model implies that I do not observe all workers employed at an establishment at a given point in time if that establishment is associated with more than 500 workers over the 1993-2008 period. However, I know the "true" number of full time employees as of June 30th in any year from the corresponding administrative variable, hence I can evaluate the fraction of workers that I do observe at each establishment. This is done in Figure A.1.1 which illustrates the ratio of the *observed* to the *actual* number of full-time employees for three different samples used in my analysis: the largest connected set, the dual-connected, and the EP sample. The actual number of full-time employees comes from the EHP, refers to June 30th each year, and is aggregated from the universe of the IEB. The observed number comes from the mover model, and refers to all workers that can be linked with a firm in a year, subject to the sample restrictions. For establishments in the largest and the dual-connected sets, I observe some 20-30% of all workers (note that the largest connected set contains the dual-connected set and the EP sample), i.e., also in establishments that never participate in the EP. For the EP sample, the coverage ratio is much higher, but it is not 100% for establishments with less than 500 employees. This is partly due to the sample processing, where I lose some connections when I define unique person-year observations. Another reason is that, by design, high turnover firms might be relatively small but still employ more than 500 workers over time.

A.1.2 Data Processing and Initial Sample Restrictions

The worker-level information is stored in continuous spells (daily accuracy) extracted from the IEB which contains the universe of employment histories of workers subject to social security (roughly 80% of total employment). The sample selection of the mover model imposes that at least one spell of any worker contained in the sample covers June 30th of some year, but conditional on doing so, the complete employment biographies for the 1993-2008 period are added to the sample. Hence, further modifications are necessary. The IEB combines employment histories of workers subject to social security with process-generated data from the Federal Employment Agency (Heining et al., 2012). The employment histories are constructed from employer notifications that detail the start and end date of a job match. An employer has to file at least one notification for all workers employed at the end of a year, and each time the salary or employment status of a worker changes. This produces a data set with multiple consecutive and potentially parallel spells (if a worker holds multiple employment relationships) within a year. In order to arrive at a sample with unique worker-establishment-year observations, several modifications are necessary. In an initial sample selection, I exclude job-spells associated with marginal employment and apprenticeships, and trim the data on location (West Germany), age (20-60), and employment status (full-time). I also exclude all non-employment spells as well as parallel employment spells by adopting the definition of the 'main job' of the IAB.

Next, I combine multiple observations with the *same* employer during a year into a single worker-establishment-year observation. Following CHK, I retain the maximum occupation code if the occupation changes between multiple worker-establishment observations with a constant establishment identifier in the same year. I compute the average daily wage for a worker-establishment-year match by weighting each wage observation with the corresponding length in days. Afterwards, I define the main job within a year as the worker-establishment match that yields the highest total earnings in a year, and drop all other job matches of that worker in the course of the same year. I deflate wages to the year 2000 using an aggregate CPI, and drop all observations with an average (real) daily wage of less than 10 Euro after collapsing the data to unique person-establishment-year combinations.

From the establishment register data, I extract three-digit industry classifications, location (district level), and the number of full-time employees as well as the number of all employees. I measure firm size by the number of full-time employees (allowing for 0) which I average over the current and the preceding year. Moreover, I modify industry and region-variables so as to be constant at the establishment-level, assigning the modal value to all establishment-year observations. The following table provides an overview of the main variables used in the analysis.

A.1.3 Imputation of Wages and Education

Wages are top-coded at the social security maximum. Top-coding affects around 10-15% of male and around 5% of female observations each year. Following CHK, I impute an upper tail of the cross-sectional wage distributions based on a series of Tobit models. Specifically, I fit a series of 560 Tobits (14 years: 1995-2008; 5 education groups: no/missing, primary, vocational, some college, university; 4 age groups: 20-29, 30-39, 40-49, 50-60; 2 genders: male and female) of the real log daily wage on a linear term in age, an individual's wage in all other years, the fraction of censored wages in all other years, an indicator for large firm (>10 employees), a quadratic polynomial in firm size, and a dummy for singleton worker observations. Then I replace the censored wage by an uncensored prediction obtained from the estimated parameters and a random draw from the corresponding truncated normal distribution.

A.2 Measures of Productivity

A.2.1 Definition of Productivity Measures

The primary measure of firm surplus used in this study is log value added, constructed from a combination of variables from the EP and administrative information from the EHP. Formally:

$$VA_{jt} = ln\left(\frac{Sales - Cost\ of\ inputs}{Firm\ size}\right)_{jt} \tag{A.1}$$

Table A.1: Description of Variables and Data Sources

Variable	Data Source	Description
Education	IEB	The education level is imputed using the IP1-procedure of Fitzenberger et al. (2006). I combine the resulting 7 education levels (including missing) into 5 categories: missing, dropouts/primary, vocational without high school ('apprenticeship'), high school with/without vocational ('some college'), polytech/university. To compute the mean years of schooling shown in Tables 2.1 and A.2, I follow CHK and assign each category the expected years of schooling required to obtain the corresponding degree: missing - 10.5 years; dropouts/primary - 11 years; apprenticeship - 13 years; some college - 15 years; university - 18 years.
Occupation	IEB	I use a three-digit occupational classification standard of 1988. It contains around 330 occupations after dropping the following categories from the analysis: 555 (Disabled), 666 (Rehabilitants), 888 (Nursing staff), 924 (Employees by household cheque procedure), 971-999 (Family assistants, unpaid interns, workforce not further specified, employees in partial retirement, among others). In the decompositions by occupations, I aggregate this measure to 9 ISCO Major Groups, combining the groups 1 (elementary) an 2 (agriculture) as well as 8 (professionals) and 9 (senior officials and managers).
Industry	EHP	I use a three-digit industry classification standard of 1993. It distinguishes between 224 industries. I combine the following industries due to very small cell sizes: 14/15, 120/131/132, 182/183, 602/603. In the associated industry-decompositions, I aggregate the three-digit classification into 16 broader categories based on a definition used in the IAB-EP.
Full-time employees	EHP	The number of full-time employees is derived from the corresponding variable in the administrative records (az_ges_vs). I interpolate the average of the past and current year, and allow for zeros.
Sales p. w.	EP	This variable is reported retrospectively for the previous year. I forward impute last year's information to obtain current year values. Sales per worker is computed by dividing through the number of full-time employees, and deflated to the year 2000 using an aggregate GDP-deflator from the World Bank. Moreover, I divide sales by 1,000.
Share of inputs	EP	This variable is also reported retrospectively, and, hence, imputed forward. It contains the percentage share of inputs in sales. I trim the variable at 1% and 99% (not percentiles!) prior to calculating value added.
Union coverage	EP	Establishments are requested to state the type of coverage, distinguishing between a) industry-wide agreements, b) firm-wide agreements, and c) no coverage. The variable is consistently available since 1995. I impute a constant union coverage by calculating the modal value of reported coverage types, and assign the stricter regime in case of ties, i.e., industry-level over firm-level over no coverage.

Note: Table summarises the main variables used in the analysis.
Source: LIAB Mover Model 9308

where *Sales* and *Cost of inputs* are derived from the EP, and *Firm size* is measured by the number of full-time employees taken from the administrative data and averaged over the past and current year. I exclude establishments in banking/insurance and public administration as their output is not measured in terms of sales. Other studies using EP data to construct productivity measures include Gürtzgen (2009, 2012), Frick (2002), and Wolf and Zwick (2002).

Cost of inputs are calculated by multiplying the reported cost share of inputs (in sales) by sales. Note that the cost share of inputs is missing in a number of cases where sales figures are available. Hence, in analyses based on sales, I can draw on a larger sample of firms compared to analyses based on value added. Most of my paper focuses on between-firm variation in log value added. I therefore average log value added per worker (and likewise log sales per worker) across firm-years, weighting by the number of person-years, $VA_j = \overline{VA_{jt}}$. A descriptive overview of the sample with mean log value added is provided in Table A.2, columns 5-6 and 11-12.

A.2.2 Descriptive Evidence of Productivity Measures

Figure A.2 shows histograms of *current* log value added per worker (panel A) and log sales per worker (panel B) in 1995 and 2008, weighting each firm equally. The distribution of log value added per worker is trimmed below the 1st and above the 99th percentile, but still reveals a sizeable dispersion: in 1995, it spans a range of about 387 thousand Euro, and in 2008 it covers some 483 thousand Euro. For log sales, the corresponding values are 1.06 million and 1.46 million Euro. These figures also illustrate the sizeable expansion of productivity dispersion over time, though I acknowledge that some of this expansion may be driven by changes in the sampling frame (note, however, that by standardising with the number of full-time employees, this concern should be reduced).

The expansion noted in Figure A.2 blends changes in the composition of firms over time (entry/exit) with time variation in productivity for continuing firms. Histograms for *average* productivity measures, i.e., which are constant at the firm level and correspond to the concept of productivity that I adopt in most of my analysis, are plotted in Figure A.3. A first observation is that the variation is smaller compared with the non-averaged case, though the overall spread is somewhat larger. It is reasonable to assume that part of the lower variation is achieved through the fact that averaging at the firm level cancels out some of the annual fluctuations that simply reflect noise. To the extent that the spikes and fatter tails in the non-averaged case (Figure A.2) are generated by fluctuations around the true productivity level of a firm, the distributions illustrated in Figure A.3 should give a better approximation of firm level productivity. A second observation is that also in the case of average productivity, which shuts of time variation in productivity for continuing firms, the distributions expand over time. Note that this is not a straightforward implication of Figure 2.2 (main text), which represents employment-weighted distributions. In that case, the changes over time might simply reflect a reallocation of workers across differently productive firms. In light of one of my main results in the paper, which is that this reallocation does not play a role in driving the growth in firm premium inequality between genders over time, it is reassuring that a similar implication follows indirectly from comparisons of the weighted and unweighted productivity distributions. Finally, note that when I distinguish between periods in the analysis, the averaging of firm productivity is conditional on whether I consider the 1995-2001 or the 2001-2008 time interval, which is consistent with allowing for unrestricted firm premiums in the AKM estimations. The histograms in contrast refer to the distribution when lumping all years together.

To understand how the productivity distribution expanded, I show in Figure A.4 the evolution of different percentiles and percentile spreads for mean log value added per worker, normalised to zero in 1995. The series now refer to the employment-weighted distributions to ensure comparability with Figure 2.2. The patterns reveal that the bulk of the overall expansion comes from a relative expansion of lower tail inequality, measured by the distance between the 10th and 50th percentile.

The IAB expanded the EP sample substantially in the year 2000, almost doubling the number of establishments in West Germany with a particular focus on small firms. It is thus reasonable to wonder whether the productivity trends shown above are merely an artefact of changes in the sampling frame. The trends shown in Figure 2.2 and Figure A.4 do not provide a strong case for this as all lines evolve rather smoothly. Of course, this is only indicative, but given theoretical as well as empirical support for a positive correlation between firm size and firm productivity (Burdett and Mortensen, 1998; Foster et al., 2008), one would suspect a disproportionate addition of small firms to imply a rather sharp drop of the lower percentiles of the distribution (although this might be alleviated to some degree by the fact that I average productivity at the firm level).

Since I can neither exclude the possibility that changes in the sample composition drive the patterns in Figure 2.2 and Figure A.4 (though, as noted above, dividing by the number of full-time employees should at least reduce this problem) or use appropriate weighting factors (see below), I resort to census data to derive a point of comparison for the growth in productivity dispersion illustrated above. While this data source has the obvious limitation of not referring to the firm level, it provides all the necessary information to compute measures of value added that are conceptually similar to the measure I use in my analysis (sales - cost of inputs). In particular, I obtained input-output tables of manufacturing industries for the four-digit level (~250 cells per year) and of all industries for the two-digit level (~70 cells per year). In each case, I deflate nominal values to the year 2010 based on a producer price index from the Federal Statistical Office, divide by the number of employees in a year (unfortunately, I cannot distinguish between full-time and part-time), take logs, and compute the employment-weighted variance. These computations lead to very similar trend increases of productivity dispersion over time. For example, between 1995 and 2007 (the last year with consistent industry codes), I find that the dispersion in manufacturing rises by about 6.4 log points and for all industries it increases by 4.4 log points. For comparison, my EP based variance of average productivity rises by 4.3 log points between 1995 and 2008. I plot the time series of the productivity variance in manufacturing in Figure 2.2.

Person-Year Weights vs. IAB Cross-Section Weights: Larger firms are overrepresented in the Establishment Panel, even after the expansion took place in 2000. To obtain results that are nationally representative one thus needs to correct for the particular sampling design. For the more widely used EP data sets, the cross-sectional models and the longitudinal models (Heining et al., 2014), the IAB provides appropriate weighting factors. These weights adjust (apart from non-response) for the disproportionate sampling within industry categories, firm size groups, and federal states, and aim to make the sample of firms in each year representative for the population of firms at the federal state level. Due to the specific sampling design of the mover model, which contains only establishments connected through worker mobility (see section A.1.1), the standard weights provided by the IAB are not useful. However, the IAB also does not provide adjusted weights.

Throughout my analysis, I therefore weight summary statistics and regressions with the number of person-years. This is a good approximation for firms with less than 500 employees, and tends to downweight larger firms in the sample, which in a sense might counterbalance the oversampling of these firms in the EP. A similar person-year weighting has been used in CHK, section VIII, in Goldschmidt and Schmieder (2017), section IV.C, and in Gürtzgen (2012), among others.

Alternative Normalisations of Productivity: The EP is currently the only data source that combines information on firm productivity with detailed worker level data. It is clear, however, that the survey nature comes along with considerable measurement error due to misreporting, roundings, and approximations on the part of respondents. To deal with this issue, I implement several cleaning steps. As regards the cost share of inputs, I drop values below 1 and above 99 percent before computing total value added. To convert total sales and value added into per capita terms, I divide by the number of full-time employees as reported in the EHP, which I previously averaged over the past and current year (more on this below). I then trim per capita measures of productivity below the 1st and above the 99th percentile, and calculate person-year weighted averages at the firm-level from the truncated distribution.

Normalising by full-time employees might produce an odd ranking of firms if some firms employ disproportionately more part-time workers than others, which in turn could affect the analysis of productivity-related gender differentials, e.g., if the share of part-time workers is particularly high in female-dominated firms. Put differently, productivity per worker might be higher in firms with a larger share of female workers because the share of females correlates negatively with the share of full-time workers. Comparing the number of full-time employees with the number of all employees (including part-time workers) in the gender-pooled sample, I find a correlation of 0.993. Moreover, a regression of the number of full-time employees on the total number of employees yields a coefficient of 0.89 with a standard error (clustered at the establishment level) of 0.023. Accordingly, switching to this variable basically generates a left-shift in the overall distribution of value added, suggesting that firms with more full-time workers employ (roughly) proportionately more part-time workers. Reassuringly, estimates for men and women yield coefficients of 0.894 and 0.852 with cluster-robust standard errors of 0.0248 and 0.0181. Hence, I cannot reject the null of no systematic upward shift in the productivity measure at female-dominated firms.[1]

A problem with the total number of employees according to the administrative data is that it is not measured consistently over time: from 1999 onwards, marginal employment became

[1]The t-test for equal coefficients gives a test statistic of 1.368.

subject to social security contributions, generating a jump in the total number of employees driven by part-time employees. Indeed, including an interaction term for the post-1999 period in the above regression yields a coefficient of -0.038 with a standard error of 0.013. Thus, it is possible that based on this variable, the productivity ranking is confounded by different numbers of marginal employees showing up in the administrative data from 1999 onwards. Again, I do not observe significant gender differences in these estimates (male coefficient: -0.0379 (0.0124); female coefficient: -0.0398 (0.0184)).

To address this consistency issue, one could draw on survey information regarding the number and structure of employees, which is even more detailed and would enable a distinction between full-time and part-time workers. Informal experimentation, however, reveals some considerable differences between the number of employees stated by employers and the number according to the administrative data. This may be due to misreporting on the part of survey respondents, e.g., if they refer to the firm rather than the establishment. Of course, one may speculate that similar misreportings prevail in the variables used to measure productivity, but ultimately, I feel more comfortable using administrative information whenever possible.

Regardless of which data source is used, the inclusion of part-time workers requires an appropriate weighting to adjust for the fewer working hours (for year-round employees) and possibly fewer days or weeks worked in a year. While the latter could be approximated by the employment biographies (daily accuracy) if one takes them as representative for all workers at an establishment (note that I do not necessarily observe all workers, in particular, at large firms), the former, and indeed far more relevant, would require to weight part-timers with an *assumed* number of hours relative to full-time workers — like in most administrative data sources, I do not observe hours of work. This is facilitated by two categories of part-time work that can be identified in the data — small and large part-time — based on the maximum hours per week, but it remains a crude approximation of the labour input that appears more appropriate at higher levels of aggregation (e.g., industries or regions), but not at the firm level.

A final questions concerns the issue whether I should use the calculated number of full-time workers using only individuals observed in the data, or the administrative information. If I observed the universe of social security records, this choice would be inconsequential. However, as I only observe a subsample of individuals, the sampling design of the mover model tends to put a lower weight on very large firms (> 500 employees) as it only guarantees a direct linkage of up to 500 workers. As the very large firms are typically more productive, this would generate longer upper tails of per capital surplus distribution. In sum, I believe that a normalisation based on the actual number of full-time workers provides the most consistent measure of firm productivity.

A.3 The Wage Bargaining Model

The log-linear wage equation with worker- and firm-fixed effects can be derived from a simple wage bargaining model. In the following paragraphs, I will provide a detailed derivation of the estimation equation. The exposition closely follows CCK.

In a given sample period p (1995-2001 or 2001-2008), the data contain N^* observations on $i = 1,\dots,N$ individuals (workers) employed at $j = 1,\dots,J$ establishments (firms). Following the notation of Abowd et al. (1999), the function $J(i,t)$ provides a mapping of worker i to firm j in year $t \in \{1,\dots,T\}$. Note that T_i may vary across i. Worker i's gender is denoted by $G(i)$ with realisations on the set $g = \{M,F\}$ as shorthand for male and female. Assume that the logarithm of the real daily wage of worker i in year t and period p who is employed at firm j is determined by (I omit the p-subscript for clarity)

$$w_{it} = a_{it} + \gamma^{G(i)} S_{i,J(i,t),t} \tag{A.2}$$

where a_{it} denotes the alternative wage available to worker i in year t, and $S_{i,J(i,t),t}$ is the contemporaneous match surplus generated through the match between worker i and firm j in year t. The coefficient γ^g measures the average share of surplus appropriated by gender g. The match surplus has the following log-linear structure:

$$S_{i,J(i,t),t} = \overline{S}_{J(i,t)} + \phi_{J(i,t),t} + \eta_{i,J(i,t)} \tag{A.3}$$

The first term represents the average surplus level at firm j. It captures long-term firm-specific components such as brand recognition or monopolistic profits from patents. ϕ_{jt} captures firm-level transitory shocks to productivity such as short-term output demand shocks and other short-run factors with instantaneous impacts on surplus. $\eta_{i,j}$ is a time-invariant match-specific component that reflects productive complementarities between worker i and firm j. I assume that $\eta_{i,j}$ is mean zero for each firm and gender in each interval. The alternative wage, a_{it} can be decomposed as follows:

$$a_{it} = \alpha_i + X_{it}'\beta^{G(i)} + \varepsilon_{it} \tag{A.4}$$

where α_i is a person-specific component that captures the idiosyncratic earnings potential which is assumed to be fully portable across employers, ε_{it} is a mean-zero worker-specific transitory earnings shock, and X_{it} is a covariate index that contains time-varying covariates that affect a worker's earnings potential irrespective of the employer that she works for. In the analysis, I include in X_{it} a set of education dummies fully interacted with year dummies, and a cubic polynomial in age (omitting the linear age term due to collinearity with year). β^g is a gender-specific vector of coefficients that contains the returns to observables. Under these assumptions, log wages can be expressed as follows:

$$w_{it} = \alpha_i + \psi^{G(i)}_{J(i,t)} + X_{it}'\beta^{G(i)} + r_{it} \tag{A.5}$$

where $r_{it} \equiv \gamma^{G(i)} \left(\phi_{J(i,t),t} + \eta_{i,J(i,t)} \right) + \varepsilon_{it}$ is a composite error term, and $\psi^{G(i)}_{J(i,t)} \equiv \gamma^{G(i)} \overline{S}_{J(i,t)}$ is a gender-specific firm-wage premium paid to all workers of gender g conditional on working at firm j. This equation, augmented by a period-specific index, is the basis for the analysis in the main text.

A.4 Exogenous Mobility and Additive Separability

While the data I use in this analysis are drawn from the same IEB source as in CHK, it is a smaller and non-random subsample of the universe of employment biographies used in their analysis. Thus, it deems worthwhile to investigate whether the basic mobility patterns are similar, and to probe the proximate validity of the exogenous mobility assumption along the way. In this section, I replicate several of the "tests" proposed by CHK and CCK, concluding that the basic assumption of exogenous mobility, as in their studies, seems to hold approximately. This finding may be useful for other researchers estimating worker-firm models since access to the universe of administrative records is often restrictive. The following discussion draws on arguments in CHK and CCK. Further useful discussions of the AKM assumptions can be found in CCHK.

A.4.1 Construction of Event Study

I begin with a sample of all male and female workers from the gender-specific largest connected sets in each interval. From this set, I select all firms that employ at least one male and one female worker over the analysis period. Then, I calculate for each worker the mean log wage of her coworkers in a given year and assign all jobs to quartiles of mean log coworker wages. Due to the sampling design, it is possible that the coworker wage is not based on all coworkers at an establishment, especially if firms are large.

Next, I identify event spells by following workers over a period of 4 years including 2 years at the old and 2 years at the new job, where the first year at the new job is coded as zero.[2] Hence, in the first period, the analysis is based on job spells beginning in 1997-2000, and in the second period it is based on job spells starting in 2003-2007. The associated transition profiles of men and women moving from the 1st and 4th quartile to any of the other quartiles are plotted in Figures A.4.1 and A.4.2.

A.4.2 Probing the Exogenous Mobility Assumption

The exogenous mobility assumption states that conditional on worker- and firm-characteristics, the probability of a match between worker i and firm j is independent of the time-varying

[2] Since job changes refer to changes in the main occupation which I define as the job that generates the maximum income within a year, I reduce the problem of long gaps in employment between two seemingly consecutive jobs. However, some job-to-job transitions may be interrupted by periods of unemployment or short-term employment at other employers. In general, the restriction to the main occupation introduces some ambiguity as to the exact timing of the transition.

error term for each gender in each period. If it is satisfied, estimation of worker- and firm-fixed effects via OLS yields unbiased parameters. To investigate possible violations of the exogenous mobility assumption, recall the definition of the composite error term of the AKM models specified in eq. (A.5):

$$r_{i,t} = \gamma^{G(i)} \left(\phi_{J(i,t),t} + \eta_{i,J(i,t)} \right) + \varepsilon_{it} \tag{A.6}$$

Consider first the term which reflects a worker's share in firm-level transitory productivity shocks, $\gamma^g \phi_{j,t}$. Under the proposed wage model, this shock affects the wages of all workers employed at a given firm, scaled by the gender-specific elasticity of wages with respect to productivity. For example, workers might be more likely to leave firms that are hit by negative productivity shocks, and join firms that are hit by positive shocks. This suggests an average wage loss just before a move occurs, and larger wage growth of recent joiners. The transition profiles in Figures A.5 and A.6 do not show evidence of such trends.

Another possible violation arises if mobility is correlated with an individual's transitory wage shock, $\varepsilon_{i,t}$. For example, workers that climb up the hierarchy in their old firm might use this momentum to enter firms that pay higher average wages. This rationale suggests a rise in mean wages prior the transition date. Inspection of the transition profiles again does not point to such trends.

Perhaps the most controversial implication of the exogenous mobility assumption is that job changes are independent of the match-specific permanent earnings component, $\eta_{i,J(i,t)}$ (Eeckhout and Kircher, 2011; Hagedorn et al., 2016). The transition profiles suggest that wage gains and losses are approximately symmetric, and that within-quartile mobility does not lead to notable wage gains. Neither observation is consistent with mobility being driven by match-specific wage components, since in that case downward and upward moves would be "more positive", leading to asymmetric transition profiles. To assess this more formally, Tables A.6 and A.7 offer a more detailed analysis of the wage profiles of men (panel A) and women (panel B). The entries in columns 1 and 2 contain the absolute and relative frequencies of moves between job quartiles. In total, the analysis in 2001-2008 is based on 262,374 job transitions of male workers and 116,863 job transitions of female workers. These transitions occur between 165,565 unique origin-destination firm combinations (not reported). Around 20-30% of all job transitions fall into each origin quartile with a minor exception being the 1st quartile among women from which 34.0% of all job transitions originate. Columns 3-6 show the mean wage profiles of movers from each gender for all origin-destination quartiles. The entries in column 7 report the 3-year log point change in wages calculated by subtracting column 6 from column 3 and multiplying by 100. In column 8, I perform a simple trend-adjustment for each origin quartile by subtracting off the 3-year raw wage change of workers who also change jobs but stay within the same job quartile. The entries in this column allow for an interpretation as the excess wage change that is associated with inter-quartile mobility beyond the return to mobility itself and conditional on heterogeneity between stayers and movers (since only movers are contained in the analysis). Conditional on this trend, it holds that wage changes associated with upward mobility are uniformly positive in sign, whereas changes associated with downward mobility are uniformly negative in sign and approximately symmetric. This holds for every origin-quartile, for every gender, and in each period.

Movers between different coworker quartiles may differ in terms of human capital. For example, young workers who just recently entered the labour market are more likely to be observed in lower coworker wage quartiles, and, depending on their education, are also more likely to transit to higher coworker quartiles later on. Such heterogeneity not only results in different initial wage levels within the same origin quartiles, but may also lead to seemingly asymmetric wage changes associated with opposite transitions. Similarly, general (economy-wide) wage gains and losses might affect the wage profiles over time. These possibilities do not pose a violation of the exogenous mobility assumption as long as the AKM models include appropriate covariates to account for such heterogeneity (X-covariates).

A cleaner picture of the wage profiles is presented in column 9, where I show regression-adjusted 3-year wage changes that account for observed heterogeneity in education and age/experience between movers and stayers. Following CCK, I regress, for each year, the 3-year wage change of stayers on dummies for the base year, education and a quadratic polynomial in age fully interacted with education dummies. I fit these models separately to the sample of stayers from each gender, and use the estimated parameters predict the associated wage change of movers with similar human capital characteristics. Finally, I subtract the predicted wage change of movers from the actual change to arrive at an excess wage growth associated with mobility between coworker quartiles. In column 10, I additionally report the corresponding two-way clustered standard errors (workers/establishments/combined) using the method of Kline (2014) which accounts for sampling errors in the regression adjustment. Overall, I find that these "cleaned" wage changes associated with *inter*-quartile mobility are sizeable and almost uniformly different from zero.

In Figure A.7, I plot mean adjusted 3-year wage changes associated with downward transitions against the corresponding upward transition for each gender. The negatively sloped 45-degree line indicates perfectly symmetric wage changes of opposing pairs of downward- and upward-mobility. Although the points do not exactly lie on a line with slope -1, they are clustered reasonably close around it, especially since this analysis is based on quartiles which leaves scope for considerable distributional effects. For example, wage changes of workers moving from 99th percentile in the 2nd quartile to the 1st percentile in the 3rd quartile are smaller (in absolute values) than wage changes of workers moving from the 99th percentile in the 3rd quartile to the 1st percentile in the 2nd quartile. If the probability of upward and downward moves depends on the location within each quartile, then this might lead to deviations from symmetry though this does not immediately imply a violation of the AKM assumption. I checked for such distributional differences by inspecting transition profiles for different percentiles (25th, 50th, 75th) within each quartile. The basic patterns were very similar.

Figures A.8 and A.9 show the regression-adjusted mean wage changes experienced by movers who move between different origin-destination quartiles relative to the wage change experienced by workers who also move but stay within the same quartile. The construction is identical to the calculations used to obtain the entries in column 8, only that I now use regression-adjusted wages. The results again confirm the proximate symmetry of gains and losses from moving between quartiles of coworker wages.

A.5 Model Fit and Additive Separability

In this section, I provide additional model fit analyses of the AKM models described in Table 2.2 of the main text. The key issue here is whether the data show signs that the additive separability assumption is not satisfied. In an initial exercise, I compute the mean AKM residuals across the joint distribution of worker- and establishment effects. The results are provided in Figures A.11 and A.12 for the two periods and genders, respectively. In general, the mean residuals are small, reflecting the relatively good fit of the wage model to the overall wage distribution (see Table 2.2). The largest deviation is -0.0084 for the 1-1 interaction among men in the first period. The fact that the AKM models perform relatively poorly for the lowest ability workers at firms with the lowest premiums has also been documented in CHK, CCK, and Macis and Schivardi (2016). In a second exercise, I calculate the event study using quartiles of estimated establishment effects rather than coworker wages.I find very similar patterns as those shown in Figures A.5 and A.6, suggesting that the wage model captures the mobility process and associated wage changes reasonably well. The results are available upon request.

A.6 Trends in Average Hours

Although my analysis focuses on full-time workers, it is still possible that variation in average working hours between men and women might overstate the gender gap. My conclusions regarding comparisons across periods are unaffected by this, unless male and female hours (of full-time workers) follow differential trends. However, an hours bias might confound the gender gap when considering each period separately. To probe into this issue, Figure A.14 illustrates the ratio of female to male usual weekly working hours calculated from OECD data. As shown by the top line (excluding part-time), the hours gap is very small (-0.9 to -1.3%), and virtually flat over the sample period. Of course, this is only suggestive since hours differences may be particularly pronounced in terms of overtime and unusual working hours (Goldin, 2014). For comparison, I also show the evolution of relative hours including part-time. In this case, the relative hours gap declines from -20% to -30% between 1995 and 2008. Even though I cannot fully rule out the presence of an hours bias for full-time workers, these figures seem to suggest that it is relatively small.

A.7 Gender-Pooled Firm Premiums

The stagnation of the overall gender wage gap on the one hand, and the rise in gender inequality due to firm-specific wages on the other hand, suggests that the wage components attributable to $X\beta$ and α contributed to a narrowing of gender wage differentials. To gauge the contribution of these components, I apply Gelbach's decomposition (Gelbach, 2016; Cardoso et al., 2016). A challenge in identifying the contributions of each component of the AKM model to the gender wage gap is that one cannot include a gender dummy in the estimations directly since it is absorbed by the person effect. Drawing on Cardoso et al. (2016), I provide a detailed derivation of how one can still identify the contributions to the gender gap based on this decomposition approach in section A.9.2.

The results of estimating Gelbach's decomposition are summarised in Table A.14, showing in even-numbered columns the estimates for the dual-connected set. Row 1 reports the unconditional gender gap, and rows 2-4 show the partition into human capital ($X\beta$), worker effects (α), and firm wage premiums (ψ). Comparing the estimates across columns, I find that the contribution of firm premiums rises from 5.4 log points (21.9%) to 7.3 log points (29.4%), yielding a cross-period growth of 1.8 log points. This is about equal to the 2.0 log points increase shown in Table 2.3, but only half the magnitude of the overall rise when introducing a bargaining channel.[3] One interpretation of this finding is that omitting the bargaining component would lead one to understate the impact of firm rent differentials on the gender gap (3.6 log points) by around 50%, suggesting that it is important to account for group-specific firm premiums in analyses of between-group inequality. Given a widening of firm rent differentials coupled with a stagnation of overall gender inequality, it is natural to wonder whether it is worker quality or human capital that counterbalances the trend in firm gender gaps, or both. Rows 3 and 4 show that both wage components would have compressed the gender gap, with human capital accounting for 1/3 and worker quality for 2/3 of the total compression.

A.8 The Impact of Children

A.8.1 Identification of Childbirth Events

In this section, I describe how I identify the event of childbirth from female employment biographies. A key advantage of my data set is the availability of raw employment biographies with daily accuracy covering the years 1993-2008. Combined with information on the reason of leave (reported by employers as part of the notification procedure), restrictions on mother's age at birth, the duration of leave, and the gap between consecutive births, this enables me to identify work interruptions that are likely due to childbirth. In particular, I require that a valid childbirth event must satisfy four main criteria: i) the reason of leave indicates a temporary

[3] The firm premium component in Table A.14 is a weighted average of the sorting components shown in Table 2.3 and Table A.8.

work interruption or maternity protection; ii) mothers are between 18 and 40 years of age; iii) the duration of leave is not below 14 weeks (the statutory maternity protection period); and iv) the gap between consecutive births is not below 40 weeks (the usual pregnancy duration). Using these restrictions, I identify about 550,000 births, of which 432,000 births (78.5%) refer to the first child.

I restrict the analysis to the birth of the first child, so that I have at most one event per year, allowing me to match the childbirth events at the person-year level with my worker sample. Doing so, however, comes along with two issues important for the interpretation. First, many women do not return to full-time employment after childbirth, while my analysis is based on full-time workers only. This might generate a selection bias, though it is not very clear in which direction this would show. For example, those returning immediately to full-time employment might be the needy who must earn money to provide their maintenance; but they could also be the privileged with high career ambitions. Second, the event of childbirth may occur throughout a year while my analysis is based on unique person-year combinations by focusing on the main job. The main job, however, may refer to the pre-birth or the post-birth period, depending on whether the birth event occurs rather early or rather late in a year. In brief, this means that the immediate (t=0) impact of childbirth on wages might confound pre- and post-birth wages, and the medium run effect (2-3 years after childbirth) may be a more representative statistic for the wage impact of children. In fact, this observation could also be one reason why the impact of children declines gradually over the first couple of years after the event date, though I presume that this pattern is dominated by the fact many mothers only return to full-time employment several years after they gave birth.

Keeping these caveats in mind, I am able to match some 196,000 births to the dual-connected sample of full-time women in their main job between 1995 and 2008. Note that the sample of "actual mothers" in the analysis is somewhat larger, consisting of about 270 thousand women (see section 2.4.6). This is because I include in this group also those women who gave birth at ages 18 and 19 (my analysis is restricted to ages 20-60), or in years 1993 and 1994 (my analysis is based on 1995-2008), or women who return to full-time years after childbirth. In these cases, where I know that a women is a mother and also know the event year, but where I do not observe the corresponding person-year combination in my main analysis sample (full-time, age 20-60, 1995-2008, dual-connected), I still include them in the regression to estimate the long run effects. This is because I consider potential wage penalties due to work interruptions and temporary hours reductions as part of the long run effect of childbirth.

A.8.2 Identifying Placebo Mothers

I follow the approach described in Appendix A of Kleven et al. (2017) for identifying a control group of non-mothers, and assigning them a placebo childbirth date. However, because my sample differs in several dimensions from the data used in Kleven et al. (2017), I have to make some adjustments which I explain in the following.

One main problem is that the data do not provide direct information on the number of children, i.e., whether a women is a mother or not. As described above, I assume that the maximum childbearing age is 40, so in a first step, I select women who were born between 1953 and 1968, i.e., who are not yet 40 by 1993 and who will have turned 40 by 2008. For some of these women, I have observed a childbirth, and for others I have not. I call the last group "never mothers", though it is conceivable that they have given birth prior to 1993, which I cannot identify using the restricted employment biographies at hand. The remaining group of mothers, which does not turn 40 until 2008, might be giving birth after the terminal year of the sample (right-censoring). To include these women in my analysis, I estimate a linear probability model for the birth cohorts 1953-1968, with the dependent variable equal to one for a never mother. I include the following list of controls: quartiles of female firm premiums and quartiles of wages (constructed for each birth cohort), education dummies, and dummies for more than 300 districts. I assign each woman of the birth cohorts 1969-1989 a predicted probability of never giving birth, and, starting from the highest predicted probability, add as many women into the control group so that the fraction of never mothers in the birth cohorts 1969-1989 is equal to the fraction of never mothers in the birth cohorts 1953-1968.

Altogether, my analysis sample thus consists of three types of women: one group that comprises of "actual mother", and two groups considered as "never mothers". The latter two groups are relevant for the identification of placebo mothers. The first of them consists of mothers born between 1953 (1993-40) and 1968 (2008-40), i.e., whose childbearing age is not right-censored (they turn 40 before 2008). This group embeds women who are actual "never mothers", plus an unknown number of women that may have given birth before 1993, hence, for whom I am unable to observe the birth date. As I cannot separately identify the latter, I keep them in the control group, noting that this might bias the childbirth penalty towards zero (their wage is already reduced due to childbirth in the past). The second group indicated above consists of women who also do not give birth between 1993 and 2008, but who only turn 40 after 2008, i.e., whose childbearing age is right-censored (and left-censored for birth cohorts 1969-1974, i.e., women who are older than 18 before 1993). For this group, which in total spans the age cohorts 1969-1989, I use the procedure described above to identify a subgroup of women who are most unlikely to give birth later in life.

For the two groups of never mothers, I then assign placebo births as in Kleven et al. (2017), using a random draw of a log normal distribution of age at first birth with location and scale parameters obtained from the associated distribution of actual mothers.

A.9 Technical Issues

A.9.1 Decompositions in Terms of Union Coverage

In this section, I derive equations (2.4) and (2.5), and show how one can estimate the contribution of gender differences in deunionisation to the male-female wage gaps. For consistency with eq. (2.2), I use notation in terms of conditional expectations instead of probability density functions. Moreover, I maintain the convention of using male employment and female returns to construct the counterfactuals.

Denote by ψ_p^g the firm premium of gender $g = \{M,F\}$ in period $p = \{1995, 2008\}$, and let U_p denote the distribution of union coverage in period p. In the following exposition, the period-subscript on the firm premiums (1995, 2008) is only added to clarify whether I refer to the first or second period. In the empirical implementation, I use the estimated parameters from the AKM models described in Table 2.2, i.e., based on 1995-2001 (here: 1995) and 2001-2008 (here: 2008).

The period p gender gap in firm premiums (FGG) can be represented as follows:

$$FGG_p = E\left[\psi_p^M | M_p, U_p\right] - E\left[\psi_p^F | F_p, U_p\right] \tag{A.7}$$

To simplify, I assume that U is binary (though, empirically, I distinguish between three types of union coverage: industry level, firm level, no coverage). Then, one can rewrite the eq. (A.7) as follows:

$$\begin{aligned}
&\underbrace{E\left[\psi_p^M | M_p, U_p = 0\right] - E\left[\psi_p^F | M_p, U_p = 0\right]}_{FGG_p^{nu}} \\
&+E\left[\psi_p^M | M_p, U_p = 0\right] + \underbrace{Pr(U_p = 1 | M_p)}_{s_{M,p}^u} \times \underbrace{\left\{E\left[\psi_p^M | M_p, U_p = 1\right] - E\left[\psi_p^M | M_p, U_p = 0\right]\right\}}_{UP_p^M} \\
&-\left(E\left[\psi_p^F | F_p, U_p = 0\right] + \underbrace{Pr(U_p = 1 | F_p)}_{s_{F,p}^u} \times \underbrace{\left\{E\left[\psi_p^F | F_p, U_p = 1\right] - E\left[\psi_p^F | F_p, U_p = 0\right]\right\}}_{UP_p^F} \right)
\end{aligned} \tag{A.8}$$

Adding and subtracting $s_{M,p}^u \times UP_p^F$ yields:

$$FGG_p = FGG_p^{nu} + \left(s_{M,p}^u - s_{F,p}^u\right) \times UP_p^F + s_{M,p}^u \times \left(UP_p^M - UP_p^F\right) \tag{A.9}$$

where $UP_p^M - UP_p^F = FGG_p^u - FGG_p^{nu}$, which corresponds to eq. (2.4) in the main text. Next, evaluate p in 1995 and 2008 and denote the difference between 2008 and 1995 by Δ:

$$\begin{aligned}
\Delta FGG = &\Delta FGG^{nu} + \left(s_{M,2008}^u - s_{F,2008}^u\right) \times UP_{2008}^F - \left(s_{M,1995}^u - s_{F,1995}^u\right) \times UP_{1995}^F \\
&+ s_{M,2008}^u \times \left(UP_{2008}^M - UP_{2008}^F\right) - s_{M,1995}^u \times \left(UP_{1995}^M - UP_{1995}^F\right)
\end{aligned} \tag{A.10}$$

Adding and subtracting the terms $\left(s^u_{M,1995} - s^u_{F,1995}\right) \times UP^F_{2008}$ and $s^u_{M,2008} \times \left(UP^M_{1995} - UP^F_{1995}\right)$ gives

$$\begin{aligned}\Delta FGG =& \Delta FGG^{nu} + \left(s^u_{M,1995} - s^u_{F,1995}\right) \times \Delta UP^F + \left(UP^M_{1995} - UP^F_{1995}\right) \Delta s^u_M \\ &+ UP^F_{2008} \Delta\left(s^u_M - s^u_F\right) + s^u_{M,2008} \Delta\left(UP^M - UP^F\right)\end{aligned} \tag{A.11}$$

where, as before, $UP^M - UP^F = FGG^u - FGG^{nu}$.

To derive the contribution of the gender bias, I begin by evaluating eq. (A.7) in 1995 and 2008, and subtracting the latter from the former. Rearranging yields:

$$\begin{aligned}\Delta FGG =& E\left[\psi^M_{2008}|M_{2008}, U_{2008}\right] - E\left[\psi^M_{1995}|M_{1995}, U_{1995}\right] \\ &- \left(E\left[\psi^F_{2008}|F_{2008}, U_{2008}\right] - E\left[\psi^F_{1995}|F_{1995}, U_{1995}\right]\right) \\ =& E\left[\psi^M_{2008} - \psi^M_{1995}|M_{1995}, U_{1995}\right] + E\left[\psi^M_{2008}|M_{2008}, U_{2008}\right] - E\left[\psi^M_{2008}|M_{1995}, U_{1995}\right] \\ &- \left(E\left[\psi^F_{2008} - \psi^F_{1995}|F_{1995}, U_{1995}\right] + E\left[\psi^F_{2008}|F_{2008}, U_{2008}\right] - E\left[\psi^F_{2008}|F_{1995}, U_{1995}\right]\right)\end{aligned} \tag{A.12}$$

Let $\Delta^1 = E\left[\psi^M_{2008} - \psi^M_{1995}|M_{1995}, U_{1995}\right] - E\left[\psi^F_{2008} - \psi^F_{1995}|F_{1995}, U_{1995}\right]$, then rearranging gives:

$$\begin{aligned}&\Delta^1 + E\left[\psi^M_{2008}|M_{2008}, U_{2008}\right] - E\left[\psi^F_{2008}|F_{2008}, U_{2008}\right] \\ &- \left(E\left[\psi^M_{2008}|M_{1995}, U_{1995}\right] - E\left[\psi^F_{2008}|F_{1995}, U_{1995}\right]\right)\end{aligned} \tag{A.13}$$

In the next step, I assign women the male unionisation rate in each period:

$$\begin{aligned}&\Delta^1 + E\left[\psi^M_{2008} - \psi^F_{2008}|M_{2008}, U_{2008}\right] + E\left[\psi^F_{2008}|M_{2008}, U_{2008}\right] - E\left[\psi^F_{2008}|F_{2008}, U_{2008}\right] \\ &- \left(E\left[\psi^M_{2008} - \psi^F_{2008}|M_{1995}, U_{1995}\right] + E\left[\psi^F_{2008}|M_{1995}, U_{1995}\right] - E\left[\psi^F_{2008}|F_{1995}, U_{1995}\right]\right)\end{aligned} \tag{A.14}$$

Denoting $\Delta^2 = E\left[\psi^M_{2008} - \psi^F_{2008}|M_{2008}, U_{2008}\right] - \left(E\left[\psi^M_{2008} - \psi^F_{2008}|M_{1995}, U_{1995}\right]\right)$, the final expression reads:

$$\begin{aligned}\Delta FGG =& \Delta^1 + \Delta^2 + \left(E\left[\psi^F_{2008}|M_{2008}, U_{2008}\right] - E\left[\psi^F_{2008}|F_{2008}, U_{2008}\right]\right) \\ &- \left(E\left[\psi^F_{2008}|M_{1995}, U_{1995}\right] - E\left[\psi^F_{2008}|F_{1995}, U_{1995}\right]\right)\end{aligned} \tag{A.15}$$

Overall, the decomposition consists of three parts. Δ^1 measures the contribution of the change in the firm gender gap ΔFGG that is due to changes in the firm-specific wage structure of men and women between 1995 and 2008, weighted by 1995 employment/unionisation rates. For example, if women experienced a relatively greater wage growth than men (unrelated to unionisation), then this would reduce the overall change in the gender wage gap by an amount of Δ^1. The second component, Δ^2, measures the contribution of shifts in male deunionisation over time, weighted by the wage structure difference of men and women in

2008.

The third part measures the contribution of gender differentials in deunionisation rates. The first term of this component, $E\left[\psi^F_{2008}|M_{2008},U_{2008}\right]-E\left[\psi^F_{2008}|F_{2008},U_{2008}\right]$, can be obtained by reweighting the female distribution with male characteristics in 2008. The empirical counterpart of this is shown in columns 4-6 of Table 2.7, with column 5 referring to the counterfactual distribution of women if they were given male union coverage rates. The second part, however, consists of one element that would require to reweight female firm premiums in 2008 with male unionisation rates in 1995. To avoid this, I approximate ψ^F_{2008} by ψ^F_{1995}. Since the distribution of female firm premiums in 1995 differs from that in 2008, this step implies that I potentially blend changes in female returns with changes in union coverage rates. Based on the results in section 2.4.3, I suspect that weighting by ψ^F_{2008} generates a larger firm premium gap, i.e., $E\left[\psi^F_{2008}|M_{1995},U_{1995}\right]-E\left[\psi^F_{2008}|F_{1995},U_{1995}\right]>E\left[\psi^F_{1995}|M_{1995},U_{1995}\right]-E\left[\psi^F_{1995}|F_{1995},U_{1995}\right]$. Hence, the approximation might overstate the contribution of the gender bias in deunionisation. However, for the sake of empirical tractability, I believe that this step is legitimate. The empirical counterpart of this second term is reported in columns 1-3 of Table 2.7.

A.9.2 Gelbach's Decomposition

In section A.7, I apply Gelbach's decomposition approach to gauge the separate contributions of worker, firm, and human capital components to the gender wage gap. In the following, I illustrate the working of this decomposition approach, and derive the final expressions. The exposition is inspired by Gelbach (2016) and Cardoso et al. (2016). As a point of departure, consider the following baseline (b) model:

$$w_{it}=X_{it}'\beta_b+W_i'\theta_b+r_{it} \tag{A.16}$$

where θ_b is a $k^c_b\times 1$ vector of returns to time-constant (c) worker characteristics (e.g., gender) stored in a $k^c_b\times 1$ vector W_i, and β_b is a $k^v_b\times 1$ vector of time-varying (v) worker observables (e.g., age interacted with education) contained in a $k^v_b\times 1$ vector $X_{i,t}$. For the exposition, I assume that $k^c_b=1$ with gender, G_i, being the only time-constant variable of interest. However, the following statements generalise to $k^c_b>1$. The assumptions so far, yield the following wage equation:

$$w_{i,t}=X_{i,t}'\beta_b+G_i\theta_b+r_{i,t} \tag{A.17}$$

In matrix notation, the stacked system can be written as:

$$w=\underset{(N^*\times k^v_b)}{X}\ \underset{(k^v_b\times 1)}{\beta_b}+\underset{(N^*\times 1)}{G}\ \underset{(1\times 1)}{\theta_b}+\underset{N^*\times 1}{r} \tag{A.18}$$

I incorporate G into X, and denote the resulting $N^*\times\left(k^v_b+1\right)$ covariate index as $\tilde{X}=[X,G]$ and the associated $\left(k^v_b+1\right)\times 1$ parameter vector as $\tilde{\beta}_b=[\beta_b,\theta_b]'$. Note that this model embeds the special case of $\tilde{X} = G$ when $k_b^v = 0$, which is the setting used in the main text. In this case, $\hat{\tilde{\beta}}_b = \hat{\theta}_b = \left(G'G\right)^{-1} G'w$ measures the unconditional gender gap, where $M_g = \left(G'G\right)^{-1} G'$ is a matrix that, if left-multiplied to a conformable vector of covariates, computes the mean difference between men and women. In general, if $k_b^v > 0$, eq. (A.18) can be reformulated as follows:

$$w = \underset{(N^* \times k_b)}{\tilde{X}} \underset{(k_b \times 1)}{\tilde{\beta}_b} + \underset{N^* \times 1}{r} \quad \text{where } \hat{\tilde{\beta}}_b = \underbrace{\left(\tilde{X}'\tilde{X}\right)^{-1} \tilde{X}'}_{M_{\tilde{X}}} w \tag{A.19}$$

Here, I use $k_b = k_b^v + k_b^c = k_b^v + 1$ to denote the total number of parameters in the baseline model. I specify the full model consistent with eq. (2.1), rewritten in matrix notation, but omit the gender-specific parameters, and instead run pooled regressions on

$$w = \underset{(N^* \times N)}{D} \underset{(N \times 1)}{\alpha} + \underset{(N^* \times J)}{F} \underset{(J \times 1)}{\psi} + \underset{(N^* \times k_f)}{X} \underset{(k_f \times 1)}{\beta_f} + \underset{(N^* \times 1)}{r} \tag{A.20}$$

where D is a $N^* \times N$ design matrix of indicators for every worker, F is a $N^* \times J$ design matrix of indicators for every establishment, and X is a $N^* \times k_f$ matrix of time-varying observables ($k_f = k_f^v$) that may (or may not) be included in eq. (A.18). This means, in particular, that the number of time-varying covariates may differ between the full and the baseline model. In this case, the full model contains a (weakly) larger number of covariates, i.e., $k_b^v \leq k_f^v$. β_f is an associated vector of pooled returns, and r is a mean zero error term. Note that in eq. (A.20), any time-invariant worker level variable — such as gender — is absorbed in the worker effect, and similarly, any time-invariant firm level variable — such as industry — is absorbed in the firm effect. Hence, one cannot include such covariates directly in the estimation.

Consider a case when $k_f^v = k_b^v > 0$ and $k_b^c = 1$, so that the baseline model and the full model both contain a positive number of time-varying observables, and the baseline model additionally includes a single time-invariant variable (gender). The 'twist' of Gelbach's decomposition is to interpret $\hat{\tilde{\beta}}_b$ in the baseline model as a biased estimator of the covariate returns on $\tilde{X}$. To illustrate, consider the fitted regression of eq. (A.20):

$$w = \underset{(N^* \times N)}{D} \underset{(N \times 1)}{\hat{\alpha}} + \underset{(N^* \times J)}{F} \underset{(J \times 1)}{\hat{\psi}} + \underset{(N^* \times k_f)}{X} \underset{(k_f \times 1)}{\hat{\beta}_f} + \underset{(N^* \times 1)}{\hat{r}} \tag{A.21}$$

Since eq. (A.20) does not contain time-invariant worker-level variables — as these parameters are absorbed in the person-fixed effects — I expand the X matrix of the full model by the time-invariant worker-level variables contained in eq. (A.18), and add a conformable number of zero rows to $\hat{\beta}_f$. Denote the corresponding components as $\tilde{X}$ and $\hat{\tilde{\beta}}_f$. Then, I left-multiply by $M_{\tilde{x}}$ to obtain:

$$\hat{\hat{\beta}}_b = \underset{(k_b \times N^*)}{M_{\tilde{x}}} \underset{(N^* \times 1)}{D\hat{\alpha}} + \underset{(k_b \times N^*)}{M_{\tilde{x}}} \underset{(N^* \times 1)}{F\hat{\psi}} + \underset{(k_b \times N^*)}{M_{\tilde{x}}} \underset{(N^* \times 1)}{\tilde{X}\hat{\hat{\beta}}_f} \tag{A.22}$$

where I use the orthogonality of $M_{\tilde{x}}$ with respect to r. Rearranging yields

$$\underset{k_b \times 1}{\hat{\hat{\beta}}_b} - \underset{k_b \times 1}{\hat{\hat{\beta}}_f} = \underset{k_b \times 1}{\hat{\delta}_{\alpha\tilde{X}}} + \underset{k_b \times 1}{\hat{\delta}_{\psi\tilde{X}}} \tag{A.23}$$

Each term on the right hand side of eq. (A.23) is a regression of the least-squares estimates of the full model on the covariate index $\tilde{X}$. Now, focusing on the row that contains the coefficient on G in each regression, one obtains:

$$\underset{1 \times 1}{\hat{\theta}_b} - \underset{1 \times 1}{\hat{\theta}_f} = \underset{1 \times 1}{\hat{\delta}_{\alpha G}} + \underset{1 \times 1}{\hat{\delta}_{\psi G}} \tag{A.24}$$

This means that the difference of the conditional gender wage gap between the baseline model and the full model can be decomposed into the sum of coefficients on the gender dummy obtained from two auxiliary regressions of the worker and firm components on $\tilde{X}$. Since $\hat{\theta}_f = 0$, this result yields an unambiguous partition of the conditional gender wage gap into worker and firm components.

In the main text, the baseline model only includes the gender dummy (and no time-varying variables) because I want to decompose the unconditional gender gap (rather than the covariate-adjusted gender gap). Therefore, consider now a baseline specification without time-varying variables, i.e., $k_b^v = 0$. Then a comparison of othe baseline and the full model additionally requires to run a regression of the fitted time-varying covariates of the full model, $X\hat{\beta}_f$, on the time-invariant component(s) contained in $\tilde{X}$. As G is the only time-invariant variable in the baseline specification, replacing $M_{\tilde{x}}$ in eq. (A.22) by M_g yields:

$$\hat{\theta}_b = \underset{(1 \times N^*)}{M_g} \underset{(N^* \times 1)}{D\hat{\alpha}} + \underset{(1 \times N^*)}{M_g} \underset{(N^* \times 1)}{F\hat{\psi}} + \underset{(1 \times N^*)}{M_g} \underset{(N^* \times 1)}{X\hat{\beta}_f} \tag{A.25}$$

In principle, the last term of eq. (A.25) specifies as many individual regressions as there are time-varying covariates in X. The total contribution of observable characteristics to the gender wage gap would then be the sum of these coefficients. However, as shown in Gelbach (2016), one can first create a heterogeneity term by summing over all covariates at the worker-year level, and then regress the resulting compound heterogeneity index on the gender dummy. This is the procedure that I implement in the main text. Using the notation from eq. (A.23), one obtains the final result:

$$\underset{1 \times 1}{\hat{\theta}_b} = \underset{1 \times 1}{\hat{\delta}_{\alpha G}} + \underset{1 \times 1}{\hat{\delta}_{\psi G}} + \underset{1 \times 1}{\hat{\delta}_{\beta G}} \tag{A.26}$$

A.10 Appendix Tables

Table A.2: Descriptive Statistics of Firms Grouped by Mean log Value Added per Worker

	1995-2001						2001-2008					
	Overall Worker Sample		Sample connected with EP-estab's		Sample with 1+ years of VA		Overall Worker Sample		Sample connected with EP-estab's		Sample with 1+ years of VA	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Observations												
Person-years	10,586,204	4,657,423	7,762,277	3,334,606	2,360,729	723,285	11,985,517	5,143,438	8,676,703	3,617,634	3,770,299	1,140,803
Percent of overall sample	*100.0*	*100.0*	*73.3*	*71.6*	*22.3*	*15.5*	*100.0*	*100.0*	*72.4*	*70.3*	*31.5*	*22.2*
Workers	1,858,422	973,038	1,552,007	764,642	517,656	180,425	1,884,818	972,199	1,560,252	750,394	707,799	247,884
Percent of overall sample	*100.0*	*100.0*	*83.5*	*78.6*	*27.9*	*18.5*	*100.0*	*100.0*	*82.8*	*77.2*	*37.6*	*25.5*
Establishments	389,253	257,083	18,247	17,397	4,731	4,485	382,775	259,279	18,777	17,936	6,930	6,578
Percent of overall sample	*100.0*	*100.0*	*4.7*	*6.8*	*1.2*	*1.7*	*100.0*	*100.0*	*4.9*	*6.9*	*1.8*	*2.5*
Number of job-spells per worker	1.60	1.47	1.12	1.09	1.03	1.03	1.61	1.49	1.10	1.08	1.04	1.03
Worker characteristics												
Mean age	39.1	37.4	40.2	38.5	40.1	38.4	41.5	39.9	42.3	40.8	41.8	40.5
Mean tenure (years)	8.4	7.2	10.0	8.6	9.4	7.9	9.4	8.0	11.6	10.1	11.1	9.4
Share low education (missing/primary)	0.14	0.18	0.15	0.20	0.17	0.28	0.12	0.15	0.13	0.16	0.14	0.21
Share medium education (apprenticeship)	0.67	0.63	0.67	0.63	0.69	0.58	0.65	0.60	0.66	0.61	0.68	0.59
Share high education (college/university)	0.19	0.18	0.18	0.17	0.14	0.14	0.23	0.25	0.22	0.23	0.18	0.20
Wages												
Mean of log daily wage	4.55	4.30	4.59	4.35	4.56	4.28	4.57	4.32	4.62	4.38	4.59	4.31
Std. dev. of log daily wage	(0.373)	(0.395)	(0.337)	(0.352)	(0.358)	(0.367)	(0.419)	(0.449)	(0.372)	(0.400)	(0.369)	(0.423)
100 x Log gender wage gap	24.8		24.0		27.9		25.0		24.1		28.0	
Percent of overall sample	*100.0*		*96.8*		*112.5*		*100.0*		*96.4*		*112.0*	
Workplace characteristics												
Mean firm size (full-time)	1,403	729	1,788	888	862	654	1,343	654	1,745	829	2,038	1,012
Share of female coworkers	0.21	0.53	0.22	0.48	0.18	0.41	0.21	0.52	0.22	0.47	0.18	0.41
Share at all-male firms	0.10	–	0.006	–	0.01	–	0.09	–	0.005	–	0.01	–
Share at all-female firms	–	0.082	–	0.005	–	0.00	–	0.080	–	0.005	–	0.00
Mean log value added p.w.					4.36	4.32					4.43	4.37
Std. dev. of mean log value added p.w.					(0.60)	(0.64)					(0.64)	(0.68)

Note: Table shows person-year weighted summary statistics for 1995-2001 and 2001-2008. The overall worker sample is the baseline sample after basic sample processing described in the main text and the Appendix. The sample connected with EP-establishments contains all workers and establishments that participate in the IAB Establishment Panel. The sample with 1+ years of mean log value added contains all establishments that participate in the EP and for which I can calculate mean log value added from available information in at least one year. The sample with mean log sales per worker is not summarised separately. Value added and sales are measured in thousands of real year 2000 Euros, using an aggregate World Bank GDP-deflator. Per capita values are computed by dividing through the number of full-time employees, and to compute "mean" productivity measures, I average annual information at the firm level within a period across all firm-years, weighting each year by total employment (person-years).
Source: LIAB Mover Model 9308

Table A.3: Descriptive Statistics of Firms Grouped by Mean log Value Added per Worker

	1995-2001			2001-2008		
	Low VA	High VA	No VA	Low VA	High VA	No VA
	(1)	(2)	(3)	(4)	(5)	(6)
Mean firm size (full-time employees)	160	341	110	157	265	101
Mean share of female employment (in %)	37.8	30.1	42.1	34.2	32.2	41.5
Industry distribution (in %)						
Agriculture, hunting, forestry, fishing	2.1	0.7	0.6	2.2	1.0	0.5
Energy and mining	0.0	0.3	0.2	0.0	0.4	0.1
Production of rubber and plastic products	2.1	4.6	2.2	3.4	3.6	1.9
Chemical industry	0.5	3.0	1.4	0.4	2.4	1.3
Metal production and processing	10.8	16.8	6.1	4.5	13.7	5.6
Automotive, production of data processing	5.1	10.3	4.6	3.8	8.4	4.2
Consumer goods	10.8	13.6	8.5	11.3	11.1	6.5
Hospitality industry	9.2	2.7	4.2	9.0	3.1	4.4
Sales (retail/wholesale)	12.8	13.3	16.3	10.5	12.8	13.8
Maintenance, repair of motor vehicles	2.6	2.1	2.0	2.6	2.6	2.0
Building industry	4.1	5.9	3.7	3.8	4.7	2.6
Transport and communication	9.2	4.6	6.0	7.1	5.5	5.9
Credit and insurance intermediation	20.0	11.8	23.6	31.6	16.2	29.0
Public and personal services	4.6	3.8	4.7	2.6	4.4	4.9
Education, social, and health care	5.6	6.2	11.4	6.8	9.6	13.0
Public administration, social security	0.5	0.3	4.4	0.4	0.4	4.4
Firm Productivity						
Avg exc. log value added per worker (1,000)	–	1.17	–	–	1.26	–
Avg mean log value added per worker (1,000)	2.88	4.37	–	2.69	4.36	–
Mean log sales per worker (in 1,000)	4.51	5.13	5.15	4.34	5.14	5.12
Observations						
# of person-years	104,579	2,804,343	10,140,115	156,747	4,489,321	10,031,367
# of firm-years	1,230	20,981	420,826	1,834	34,011	479,099
# of firms	195	3,196	85,577	266	4,643	88,397

Note: Statistics are based on the dual-connected set. Each firm enters with weight 1. Columns 1-2 and 4-5 refer to firms for which value added is observed in the data. Firms are grouped into "low" and "high" according to the critical threshold value obtained from the non-linear system of equations (see main text). Entries in columns 3 and 6 show statistics for firms with no value added. Some of these establishments provide information on sales. Value added and sales are measured in thousands of real year 2000 Euros, using a World Bank GDP-deflator.
Source: LIAB Mover Model 9308

Table A.4: Summary Statistics of Samples Used in Stayer Analysis

	Men (1)	Women (2)
Worker characteristics		
3-year wage change	0.06	0.07
3- year lagged wage level	4.54	4.27
3-year lagged age	39.9	39.0
3-year lagged female employment share	0.17	0.36
Excess log value added per worker	1.29	1.27
Education shares (in percent)		
Low education (missing/primary)	17.9	29.0
Medium education (apprenticeship)	67.5	55.7
High education (some college/university)	14.6	15.4
Industry employment shares (in percent)		
Agriculture, hunting, forestry, fishing	0.3	0.2
Energy and mining	0.9	0.3
Production of rubber and plastic products	5.6	4.3
Chemical industry	6.1	7.6
Metal production and processing	32.1	19.6
Automotive, production of data processing	17.5	16.3
Consumer goods	12.2	15.8
Hospitality industry	0.2	0.6
Sales (retail/wholesale)	4.2	9.5
Maintenance, repair of motor vehicles	1.5	1.3
Building industry	6.3	2.7
Transport and communication	3.3	1.9
Credit and insurance intermediation	4.6	9.1
Public and personal services	4.0	3.0
Education, social, and health care	1.0	7.3
Public administration, social security	0.1	0.5
Observations		
Number of person-years	639,644	153,121
Number of establishments	2,371	2,371

Note: Table shows summary statistics of men and women who stay with their employer for at least three years between 1995 and 2008, and whose employer has non-missing excess log value added over the entire time frame. Value added is measured in thousands of real year 2000 Euros, using an aggregate World Bank GDP-deflator.
Source: LIAB Mover Model 9308

Table A.5: Summary of Estimation Results for Additional AKM Models

	Gender-Pooled				Period-Pooled			
	1995-2001		2001-2008		Men		Women	
	(1)		(2)		(3)		(4)	
Estimation results								
# of person-effects	2,744,864		2,769,838		2,070,621		1,128,590	
# of firm-effects	436,807		426,126		542,993		331,429	
Standard dev. of person-effects (a)	0.290		0.318		0.275		0.285	
Standard dev. of firm-effects (y)	0.160		0.199		0.181		0.222	
Standard dev. of covariates (Xb)	0.095		0.077		0.110		0.087	
Correlation of person- and firm-effects	0.101		0.135		0.123		0.043	
Correlation of male and female firm-effects					0.713			
Model fit								
Adjusted R2 of AKM model	0.911		0.919		0.901		0.858	
Root MSE of AKM model	0.103		0.112		0.117		0.144	
Fit of match effects model								
Adjusted R2 of match-effects model	0.930		0.935		0.929		0.889	
Root MSE of match-effects model	0.104		0.111		0.106		0.139	
Standard dev. of match-effects	0.055		0.059		0.069		0.077	
in % of residual variance of AKM	*(28.7)*		*(27.4)*		*(34.8)*		*(28.4)*	
Variance decomposition								
Var(w)	0.152	*100*	0.191	*100*	0.158	*100*	0.174	*100*
Var(α)	0.084	*55*	0.101	*53*	0.076	*48*	0.081	*47*
Var(ψ)	0.026	*17*	0.040	*21*	0.033	*21*	0.049	*28*
Var($X\beta$)	0.009	*6*	0.006	*3*	0.012	*8*	0.008	*4*
$2 \times$ Cov(α, ψ)	0.009	*6*	0.017	*9*	0.012	*8*	0.005	*3*
$2 \times$ Cov(α, $X\beta$)	0.010	*7*	0.011	*6*	0.008	*5*	0.008	*4*
$2 \times$ Cov(ψ, $X\beta$)	0.003	*2*	0.004	*2*	0.003	*2*	0.002	*1*
Var(r)	0.011	*7*	0.013	*7*	0.014	*9*	0.021	*12*

Note: Table shows results from OLS estimations of AKM models specified in eq. (2.1) for gender-pooled samples (columns 1-2), and for period-pooled samples (columns 3-4). All models are estimated on the largest connected sets as defined in the main text. The correlation of male and female firm effects is calculated for the subset of dual-connected firms. Match effect models contain a dummy for each job-match. The correlations of fixed effects between the pooled (P) and the gender-specific (M, F) models, summarised in Table 2.2, are as follows. For the 1990s: Corr(ψ^M,ψ^P) = 0.9488, Corr(ψ^F,ψ^P) = 0.8139, Corr(α^M,α^P) = 0.9740, Corr(α^F,α^P) = 0.9362; for the 2000s: Corr(ψ^M,ψ^P) = 0.9646, Corr(ψ^F,ψ^P) = 0.8572, Corr(α^M,α^P) = 0.9820, Corr(α^F,α^P) = 0.9432
Source: LIAB Mover Model 9308

Table A.6: Mean Log Real Daily Wages of Movers Between Origin- and Destination Quartiles in 1995-2001

			Mean log daily wage of movers				3-Year Wage Change (in percent)			
Origin-Destin. Quartile	Nb. of moves (1)	Perc. of moves (2)	2 years prior (3)	1 year prior (4)	1 year post (5)	2 years post (6)	Raw (7)	Trend-adjusted (8)	Regr.-adjusted (9)	Stand. Error (10)
Panel A. Men										
1 to 1	32,722	53.9	4.230	4.232	4.253	4.272	4.2	0.0	-1.1	(0.3)
1 to 2	14,769	24.3	4.350	4.350	4.445	4.464	11.4	7.1	5.6	(1.3)
1 to 3	7,824	12.9	4.322	4.332	4.501	4.556	23.4	19.2	15.8	(0.6)
1 to 4	5,371	8.9	4.407	4.425	4.645	4.715	30.8	26.6	21.4	(0.7)
2 to 1	9,061	18.0	4.435	4.438	4.382	4.411	-2.4	-6.9	-8.1	(0.5)
2 to 2	20,766	41.3	4.492	4.497	4.520	4.537	4.5	0.0	-0.3	(0.5)
2 to 3	14,515	28.9	4.550	4.560	4.613	4.637	8.7	4.2	2.6	(0.5)
2 to 4	5,882	11.7	4.646	4.662	4.775	4.835	18.9	14.4	9.6	(0.5)
3 to 1	5,181	10.7	4.518	4.530	4.418	4.444	-7.4	-14.4	-13.5	(0.8)
3 to 2	7,794	16.1	4.584	4.600	4.603	4.629	4.6	-2.5	-1.7	(0.6)
3 to 3	22,200	45.9	4.638	4.647	4.675	4.708	7.0	0.0	1.2	(0.4)
3 to 4	13,223	27.3	4.723	4.739	4.824	4.875	15.2	8.2	6.7	(0.5)
4 to 1	3,429	4.3	4.644	4.661	4.510	4.546	-9.8	-18.9	-16.7	(1.2)
4 to 2	4,199	5.2	4.730	4.752	4.721	4.763	3.2	-5.8	-4.3	(0.5)
4 to 3	10,261	12.8	4.757	4.783	4.781	4.822	6.4	-2.6	-0.8	(0.5)
4 to 4	62,501	77.7	4.875	4.896	4.928	4.965	9.1	0.0	2.2	(0.3)
Panel B. Women										
1 to 1	23,575	60.5	4.086	4.095	4.123	4.138	5.2	0.0	-0.6	(0.4)
1 to 2	8,439	21.6	4.218	4.229	4.325	4.345	12.8	7.6	6.3	(0.5)
1 to 3	4,099	10.5	4.187	4.198	4.356	4.396	21.0	15.8	13.3	(0.8)
1 to 4	2,880	7.4	4.219	4.236	4.462	4.517	29.8	24.6	21.0	(0.8)
2 to 1	5,019	22.8	4.280	4.283	4.231	4.251	-2.9	-8.7	-9.2	(0.6)
2 to 2	8,957	40.7	4.358	4.372	4.402	4.417	5.9	0.0	-0.5	(0.3)
2 to 3	5,653	25.7	4.401	4.415	4.465	4.485	8.4	2.5	1.2	(0.4)
2 to 4	2,382	10.8	4.437	4.455	4.566	4.608	17.2	11.3	8.5	(0.6)
3 to 1	2,395	5.2	4.325	4.337	4.246	4.271	-5.4	-13.5	-12.2	(0.8)
3 to 2	32,268	70.3	4.388	4.406	4.434	4.454	6.7	-1.3	-0.3	(0.5)
3 to 3	6,969	15.2	4.438	4.455	4.495	4.519	8.0	0.0	1.1	(0.4)
3 to 4	4,271	9.3	4.529	4.553	4.636	4.673	14.4	6.4	5.9	(0.5)
4 to 1	1,606	6.1	4.422	4.442	4.299	4.343	-8.0	-18.8	-15.3	(1.0)
4 to 2	1,657	6.3	4.464	4.488	4.478	4.508	4.4	-6.4	-3.2	(0.7)
4 to 3	3,568	13.6	4.559	4.575	4.599	4.631	7.9	-3.0	0.1	(0.5)
4 to 4	19,423	74.0	4.663	4.696	4.747	4.771	10.8	0.0	3.4	(0.3)

Note: Sample contains all event spells defined as job transitions with 2+ preceding and 2+ succeeding observations at the old and new employer, respectively. Origin-destination quartiles are constructed from co-worker wages, focusing on firms in the dual-connected set. Firms that, in a given year, can be linked to a single worker only ("singleton-years") are excluded from the analysis. Columns 1 and 2 show the distribution of even spells by origin-destination quartile indicated in the row-heading. Entries in columns 3 to 6 are mean log real daily wages of movers around the transition date. Columns 7 to 9 report the (approximate) percent changes of wages for movers comparing two years prior and two years after the event. Trend-adjusted changes are constructed by subtracting the mean wage change of movers who move between the same origin-destination quartile (1-1, 2-2,...) during the period. Regression-adjusted wage changes are computed by fitting a linear regression separately to male and female stayers (model includes 5 education dummies fully interacted with a quadratic term in age, and a dummy for the initial year), and using the coefficients of that model to predict the wage change of movers. Column 9 reports twoway-clustered standard errors (workers/establishments/combined) of the predicted wage change using the method of Kline (2014). This accounts for sampling errors in the regression adjustment. The SAS/Stata routines for this analysis were provided by Patrick Kline.
Source: LIAB Mover Model 9308

Table A.7: Mean Log Real Daily Wages of Movers Between Origin- and Destination Quartiles in 2001-2008

			Mean log daily wage of movers				3-Year Wage Change (in percent)			
Origin-Destin. Quartile	Nb. of moves (1)	Perc. of moves (2)	2 years prior (3)	1 year prior (4)	1 year post (5)	2 years post (6)	Raw (7)	Trend-adjusted (8)	Regr.-adjusted (9)	Stand. Error (10)
Panel A. Men										
1 to 1	44,528	68.1	4.156	4.151	4.131	4.139	-1.6	0.0	-5.0	(0.3)
1 to 2	10,543	16.1	4.324	4.327	4.437	4.455	13.0	14.7	9.0	(0.7)
1 to 3	6,071	9.3	4.315	4.329	4.522	4.562	24.7	26.3	19.5	(0.9)
1 to 4	4,219	6.5	4.377	4.400	4.650	4.706	32.9	34.5	26.9	(1.0)
2 to 1	13,094	23.0	4.445	4.445	4.292	4.305	-14.0	-14.7	-17.2	(0.6)
2 to 2	26,030	45.8	4.548	4.544	4.555	4.555	0.7	0.0	-2.0	(0.3)
2 to 3	11,968	21.0	4.646	4.649	4.684	4.700	5.4	4.7	1.4	(0.4)
2 to 4	5,800	10.2	4.755	4.771	4.845	4.892	13.7	13.0	8.1	(0.7)
3 to 1	7,036	11.9	4.542	4.544	4.302	4.329	-21.4	-24.7	-25.4	(0.8)
3 to 2	10,369	17.5	4.674	4.688	4.652	4.671	-0.3	-3.6	-4.2	(0.4)
3 to 3	26,367	44.6	4.711	4.720	4.738	4.744	3.3	0.0	-0.3	(0.3)
3 to 4	15,395	26.0	4.824	4.833	4.884	4.925	10.1	6.8	5.1	(0.4)
4 to 1	4,202	5.2	4.692	4.707	4.425	4.463	-22.9	-28.8	-27.4	(1.3)
4 to 2	4,740	5.9	4.828	4.862	4.776	4.813	-1.5	-7.5	-6.3	(0.8)
4 to 3	11,823	14.6	4.863	4.881	4.851	4.886	2.3	-3.6	-2.2	(0.4)
4 to 4	60,189	74.3	4.988	5.004	5.006	5.047	5.9	0.0	1.5	(0.2)
Panel B. Women										
1 to 1	28,373	71.3	4.066	4.059	4.038	4.036	-2.9	0.0	-5.1	(0.3)
1 to 2	6,167	15.5	4.230	4.230	4.327	4.334	10.4	13.3	7.4	(0.8)
1 to 3	3,029	7.6	4.213	4.216	4.372	4.400	18.7	21.6	14.6	(0.9)
1 to 4	2,211	5.6	4.217	4.235	4.463	4.500	28.3	31.2	23.4	(1.2)
2 to 1	6,639	25.3	4.317	4.311	4.185	4.189	-12.8	-12.7	-15.4	(0.7)
2 to 2	11,641	44.3	4.426	4.421	4.434	4.424	-0.2	0.0	-2.5	(0.4)
2 to 3	5,543	21.1	4.483	4.481	4.524	4.525	4.2	4.4	0.8	(0.5)
2 to 4	2,455	9.3	4.539	4.547	4.623	4.650	11.2	11.3	6.7	(0.8)
3 to 1	3,587	14.9	4.376	4.370	4.174	4.189	-18.7	-20.3	-21.8	(0.9)
3 to 2	4,368	18.2	4.511	4.516	4.496	4.506	-0.5	-2.1	-3.9	(0.4)
3 to 3	10,917	45.4	4.554	4.561	4.580	4.570	1.6	0.0	-1.1	(0.4)
3 to 4	5,149	21.4	4.634	4.641	4.701	4.721	8.7	7.1	4.7	(0.5)
4 to 1	2,109	7.9	4.507	4.510	4.222	4.248	-25.9	-31.8	-29.3	(1.1)
4 to 2	1,981	7.4	4.626	4.638	4.577	4.593	-3.2	-9.2	-7.0	(0.7)
4 to 3	4,196	15.7	4.647	4.664	4.640	4.653	0.6	-5.3	-3.1	(0.5)
4 to 4	18,498	69.1	4.802	4.819	4.843	4.861	6.0	0.0	2.4	(0.3)

Note: See notes to Table A.6.
Source: LIAB Mover Model 9308

Table A.8: Decomposition of Changes in the Gender Gap of Firm Premiums Across Periods – Reversing the Reference Group

	1995-2001				2001-2008				Differences			
	Male	Female	Δ Male-Female		Male	Female	Δ Male-Female		Male	Female	Δ Male-Female	
			Change	Share			Change	Share			Change	Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. All workers												
Mean log real daily wages (unadjusted)			0.247	100.0			0.247	100.0			0.000	0.0
Firm-specific wage premium	0.103	0.075	0.028	11.3	0.176	0.112	0.064	25.9	0.073	0.037	0.036	14.6
Sorting (using female FE's)	0.137	0.075	0.062	25.1	0.189	0.112	0.077	31.1	0.052	0.037	0.015	6.0
Bargaining (using male employment)	0.103	0.137	-0.034	-13.6	0.176	0.189	-0.013	-5.3	0.073	0.053	0.021	8.3
Panel B. Skill Groups												
i. Missing / Primary												
Mean log real daily wages (unadjusted)			0.223	100.0			0.226	100.0			0.003	1.3
Firm-specific wage premium	0.093	0.068	0.024	10.9	0.151	0.095	0.056	24.9	0.058	0.027	0.032	12.9
Sorting (using female FE's)	0.119	0.068	0.051	22.9	0.156	0.095	0.061	26.9	0.036	0.027	0.010	4.0
Bargaining (using male employment)	0.093	0.119	-0.027	-12.0	0.151	0.156	-0.005	-2.0	0.058	0.036	0.022	9.0
ii. Apprenticeship												
Mean log real daily wages (unadjusted)			0.204	100.0			0.201	100.0			-0.002	-1.2
Firm-specific wage premium	0.103	0.074	0.028	13.9	0.173	0.109	0.063	31.5	0.070	0.035	0.035	14.2
Sorting (using female FE's)	0.137	0.074	0.063	30.7	0.186	0.109	0.076	38.0	0.049	0.035	0.014	5.6
Bargaining (using male employment)	0.103	0.137	-0.034	-16.8	0.173	0.186	-0.013	-6.5	0.070	0.049	0.021	8.6
iii. College / University												
Mean log real daily wages (unadjusted)			0.276	100.0			0.292	100.0			0.016	5.7
Firm-specific wage premium	0.117	0.096	0.021	7.6	0.195	0.136	0.059	20.3	0.079	0.040	0.038	15.5
Sorting (using female FE's)	0.161	0.096	0.066	23.8	0.220	0.136	0.085	29.0	0.059	0.040	0.019	7.6
Bargaining (using male employment)	0.117	0.161	-0.045	-16.3	0.195	0.220	-0.025	-8.7	0.079	0.059	0.019	7.9

Note: Sample contains all firms in dual-connected sets in each sample period. Columns 1-4 show results for the 1990s, columns 5-8 for the 2000s, and columns 9-12 calculate changes across periods. In each set of columns, the first two columns calculate the (counterfactual) levels of firm rents using eq. (2.2). The next column calculates the difference, and the last column reports percent shares of row 1 in each panel. Entries in column 12 refer to the percent change relative to the level in the 1990s. For example, 3.6/24.7 = 14.6%.
Source: LIAB Mover Model 9308

Table A.9: Decomposition of Changes in the Gender Gap of Firm Premiums Across Periods – Alternative Normalisations

	1995-2001				2001-2008				Differences			
	Male	Female	Δ Male-Female		Male	Female	Δ Male-Female		Male	Female	Δ Male-Female	
			Change	Share			Change	Share			Change	Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Mean log value added per worker (Table 3)												
Mean log real daily wages (unadjusted)			0.247	100.0			0.247	100.0			0.000	0.0
Firm-specific wage premium	0.103	0.075	0.028	11.3	0.176	0.112	0.064	25.9	0.073	0.037	0.036	14.6
Sorting (using female FE's)	0.118	0.075	0.042	17.2	0.175	0.112	0.063	25.4	0.057	0.037	0.020	8.3
Bargaining (using male employment)	0.103	0.117	-0.014	-5.8	0.176	0.175	0.001	0.3	0.073	0.058	0.015	6.1
Panel B: Mean log sales per worker (bottom 6%)												
Mean log real daily wages (unadjusted)			0.247	100.0			0.247	100.0			0.000	0.0
Firm-specific wage premium	0.130	0.086	0.044	17.9	0.261	0.196	0.065	26.3	0.131	0.110	0.021	8.4
Sorting (using female FE's)	0.128	0.086	0.042	17.2	0.259	0.196	0.063	25.4	0.131	0.110	0.020	8.3
Bargaining (using male employment)	0.130	0.129	0.001	0.6	0.261	0.259	0.002	0.8	0.131	0.130	0.001	0.2
Panel C: Hospitality Industry												
Mean log real daily wages (unadjusted)			0.247	100.0			0.247	100.0			0.000	0.0
Firm-specific wage premium	0.289	0.244	0.045	18.2	0.300	0.249	0.051	20.6	0.011	0.005	0.006	2.4
Sorting (using female FE's)	0.287	0.244	0.043	17.2	0.312	0.249	0.063	25.4	0.025	0.005	0.020	8.2
Bargaining (using male employment)	0.289	0.287	0.002	1.0	0.300	0.312	-0.012	-4.8	0.011	0.025	-0.014	-5.8
Panel D: Business Service Providers												
Mean log real daily wages (unadjusted)			0.247	100.0			0.247	100.0			0.000	0.0
Firm-specific wage premium	0.185	0.162	0.023	9.1	0.269	0.199	0.070	28.4	0.084	0.037	0.047	19.2
Sorting (using female FE's)	0.205	0.162	0.042	17.2	0.262	0.199	0.063	25.4	0.057	0.037	0.020	8.2
Bargaining (using male employment)	0.185	0.205	-0.020	-8.0	0.269	0.261	0.007	3.0	0.084	0.057	0.027	11.0

Note: See notes to Table 2.3 and main text for descriptions of the alternative normalisations schemes.
Source: LIAB Mover Model 9308

Table A.10: Decomposition of the Gender Gap in Firm Premiums by Occupations in the 1990s and 2000s

	1995-2001				2001-2008			
	Overall gender wage gap	Gender gap in firm premiums	Decomposition		Overall gender wage gap	Gender gap in firm premiums	Decomposition	
			Sorting Effect	Bargaining Effect			Sorting Effect	Bargaining Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All workers	0.247	0.028	0.042	-0.014	0.247	0.064	0.063	0.001
		(11.4)	(17.2)	(-5.8)		(25.8)	(25.4)	(0.3)
Occupation: ISCO88 major groups								
Elementary/agricultural occupations	0.288	0.032	0.054	-0.022	0.279	0.059	0.067	-0.008
		(11.1)	(18.8)	(-7.7)		(21.0)	(24.0)	(-3.0)
Plant/machine operator	0.275	-0.005	0.004	-0.009	0.261	0.014	0.012	0.002
		(-1.7)	(1.5)	(-3.2)		(5.3)	(4.6)	(0.7)
Craft and related workers	0.367	0.067	0.069	-0.002	0.364	0.078	0.063	0.015
		(18.4)	(18.9)	(-0.5)		(21.4)	(17.4)	(4.1)
Service/sales workers	0.224	-0.016	0.010	-0.026	0.232	0.033	0.029	0.004
		(-6.9)	(4.6)	(-11.5)		(14.3)	(12.4)	(1.9)
Clerks	0.221	-0.005	0.022	-0.023	0.235	0.024	0.031	-0.007
		(-2.3)	(10.0)	(-10.2)		(10.3)	(13.3)	(-3.0)
Technicians/Associate Professionals	0.310	0.026	0.043	-0.017	0.326	0.070	0.069	0.001
		(8.4)	(13.8)	(-5.4)		(21.5)	(21.2)	(0.3)
Professionals/Senior officials/Managers	0.361	0.043	0.068	-0.025	0.382	0.103	0.112	-0.009
		(12.0)	(18.8)	(-6.8)		(27.0)	(29.3)	(-2.3)

Note: Sample contains all firms in the dual-connected set in 1995-2001. Occupation groups indicated in the row heading are aggregates from three digit occupational codes into ISCO88 major groups (categories 1 and 2 as well as 8 and 9 are each merged into a single group). Column 1 shows the overall gender gap conditional on the group indicated in the row heading. Column 2 shows the difference in normalised gender-specific firm effects. Entries in columns 3-4 and 7-8 refer to the sorting and bargaining components, where sorting effects are evaluated using female firm premiums and bargaining effects using male employment. Values in parentheses are percentage shares of the overall gender gap in column 1.
Source: LIAB Mover Model 9308

Table A.11: Decomposition of the Gender Gap in Firm Premiums by Industries in the 1990s and 2000s

	1995-2001				2001-2008			
	Overall gender wage gap	GG in firm rents	Decomposition		Overall gender wage gap	GG in firm rents	Decomposition	
			Sorting Effect	Bargaining Effect			Sorting Effect	Bargaining Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All workers	0.247	0.028	0.042	-0.014	0.247	0.064	0.063	0.001
		(11.4)	(17.2)	(-5.8)		(25.8)	(25.4)	(0.3)
Industry Groups (aggregates of 3-digit codes)								
Agriculture, hunting, forestry, fishing	0.258	0.074	0.081	-0.007	0.273	0.053	0.113	-0.059
		(28.9)	(31.6)	(-2.6)		(19.6)	(41.2)	(-21.6)
Energy and mining	0.070	0.033	0.016	0.017	0.038	0.002	0.000	0.002
		(47.6)	(22.9)	(24.7)		(6.6)	(1.1)	(5.5)
Production of rubber and plastic products	0.271	0.017	0.028	-0.011	0.266	0.049	0.041	0.007
		(6.3)	(10.3)	(-3.9)		(18.3)	(15.5)	(2.8)
Chemical industry	0.188	-0.010	0.014	-0.024	0.164	0.006	0.018	-0.012
		(-5.4)	(7.3)	(-12.6)		(3.8)	(11.1)	(-7.3)
Metal production and processing	0.248	0.003	-0.004	0.007	0.239	0.011	0.007	0.004
		(1.2)	(-1.5)	(2.6)		(4.6)	(3.1)	(1.5)
Automotive, production of data processing	0.308	0.009	0.046	-0.038	0.286	0.037	0.029	0.008
		(2.8)	(15.0)	(-12.2)		(12.9)	(10.0)	(2.9)
Consumer goods	0.314	0.057	0.054	0.004	0.295	0.080	0.054	0.026
		(18.3)	(17.0)	(1.3)		(27.0)	(18.2)	(8.8)
Hospitality industry	0.219	-0.005	0.011	-0.015	0.221	0.024	0.011	0.013
		(-2.2)	(4.8)	(-7.0)		(11.0)	(5.2)	(5.9)
Sales (retail/wholesale)	0.284	0.023	0.012	0.011	0.271	0.043	0.017	0.026
		(8.1)	(4.1)	(4.0)		(15.8)	(6.2)	(9.6)
Maintenance, repair of motor vehicles	0.243	0.041	0.032	0.008	0.238	0.039	0.012	0.027
		(16.8)	(13.3)	(3.5)		(16.4)	(5.1)	(11.3)
Building industry	0.240	0.101	0.013	0.088	0.235	0.104	0.010	0.094
		(42.2)	(5.4)	(36.8)		(44.3)	(4.3)	(40.1)
Transport and communication	0.181	0.011	0.013	-0.002	0.165	0.022	0.035	-0.012
		(6.1)	(7.4)	(-1.3)		(13.6)	(21.1)	(-7.6)
Credit and insurance intermediation	0.222	-0.028	-0.006	-0.022	0.180	-0.023	-0.011	-0.012
		(-12.4)	(-2.6)	(-9.8)		(-12.7)	(-6.1)	(-6.6)
Public and personal services	0.243	0.020	0.037	-0.017	0.242	0.045	0.051	-0.006
		(8.3)	(15.4)	(-7.2)		(18.5)	(20.9)	(-2.4)
Education, social, and health care	0.217	-0.068	-0.005	-0.063	0.240	-0.031	0.005	-0.026
		(-31.3)	(-2.2)	(-29.1)		(-12.8)	(2.1)	(-10.7)
Public administration, social security	0.143	-0.070	-0.003	-0.068	0.128	-0.042	0.004	-0.046
		(-49.4)	(-2.0)	(-47.4)		(-32.7)	(3.2)	(-35.9)

Note: Sample contains all firms in the dual-connected set in 1995-2001. Industries are aggregates of 3-digit industry classifications. Columns 1 and 5 show the overall gender gap conditional on the group indicated in the row heading. Columns 2 and 6 show the gender gap in normalised gender-specific firm effects. Entries in columns 3 and 7 show the sorting effects using female firm effects. Entries in columns 4 and 8 show the corresponding bargaining effects using male employment. Values in parentheses represent percentage shares of the corresponding component relative to the overall gender gap in column 1.
Source: LIAB Mover Model 9308

Table A.12: Summary of AKM Models for Low/High Union Coverage Industries and Decomposition of Firm Gender Wage Gaps

	Below Median Union Coverage				Above Median Union Coverage			
	1995-2001		2001-2008		1995-2001		2001-2008	
	Men	Women	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Summary of largest connected sets and AKM models								
i. Observations								
Overall	6,656,101	3,131,508	7,856,050	3,506,962	4,022,936	1,663,767	4,404,665	1,757,217
Largest connected set	6,470,272	2,896,927	7,637,726	3,229,079	3,900,287	1,522,286	4,248,275	1,596,072
Percentage share	*97.2*	*92.5*	*97.2*	*92.1*	*97.0*	*91.5*	*96.4*	*90.8*
ii. Descriptive Statistics								
Mean log daily wage	4.557	4.287	4.577	4.305	4.632	4.415	4.686	4.471
Mean age	38.9	37.3	41.3	39.9	40.0	38.1	42.5	40.6
iii. Estimation results								
# of person-effects	1,226,555	631,779	1,285,048	634,053	749,920	330,926	727,677	316,690
# of firm-effects	182,922	123,995	191,893	126,274	34,058	21,572	33,495	21,586
Standard dev. of person-effects (a)	0.283	0.303	0.315	0.340	0.270	0.276	0.290	0.317
Standard dev. of firm-effects (y)	0.164	0.208	0.208	0.245	0.143	0.170	0.172	0.209
Standard dev. of covariates (Xb)	0.106	0.078	0.083	0.068	0.105	0.078	0.086	0.070
Correlation of person- and firm-effects	0.039	-0.086	0.098	-0.050	-0.162	-0.237	-0.155	-0.287
Panel B. Baseline decompositions for dual-connected set								
Threshold identifying zero surplus firms	3.15		3.15		3.10		3.25	
Gender gap in log daily wages	0.270		0.272		0.217		0.216	
	(100.0)		(100.0)		(100.0)		(100.0)	
Gender gap in firm-specific wage premiums	0.025		0.115		0.069		-0.018	
	(9.2)		(42.3)		(31.7)		(-8.4)	
Sorting (using female FE's)	0.030		0.055		0.054		0.067	
	(11.1)		(20.3)		(25.0)		(31.3)	
Bargaining (using male employment)	-0.005		0.060		-0.012		-0.086	
	(-2.0)		(22.0)		(-5.5)		(-39.7)	

Note: Panel A summarises AKM models estimated for firms/workers in the largest connected sets of industries with below (columns 1-4) or above (columns 5-8) median union coverage as of 2010. The grouping is based on information on union coverage from Statistical Yearbooks. Panel B shows results of the associated decomposition of firm premiums as specified in eq. (2.2), where I previously normalised the AKM firm effects based on the identified threshold values indicated in the first row using the CCK procedure.
Source: LIAB Mover Model 9308; Federal Statistical Office

Table A.13: Decomposition of Rent-Sharing Elasticities

	Model 1		Model 2		Model 3	
	Overall	Fraction	Overall	Fraction	Overall	Fraction
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. 1995-2001						
Log Real Daily Wage	0.156	100.0	0.131	100.0	0.097	100.0
	(0.010)		(0.009)		(0.007)	
Person Effects	0.066	42.2	0.057	43.1	0.042	43.2
	(0.005)		(0.005)		(0.004)	
Establishment Effects	0.089	57.0	0.074	56.0	0.054	55.9
	(0.006)		(0.005)		(0.005)	
Covariates	0.001	0.8	0.001	0.8	0.001	0.9
	(0.000)		(0.000)		(0.000)	
# of observations			2,674,024			
# of workers			603,961			
# of establishments			4,115			
Panel B. 2001-2008						
Log Real Daily Wage	0.195	100.0	0.147	100.0	0.105	100.0
	(0.014)		(0.010)		(0.008)	
Person Effects	0.066	33.7	0.060	40.8	0.045	43.5
	(0.006)		(0.005)		(0.005)	
Establishment Effects	0.128	65.6	0.086	58.5	0.059	56.1
	(0.011)		(0.006)		(0.005)	
Covariates	0.001	0.7	0.001	0.7	0.000	0.4
	(0.000)		(0.000)		(0.000)	
# of observations			4,134,895			
# of workers			812,767			
# of establishments			6,027			

Note: Table shows decomposition of firm-level rent-sharing elasticities into worker, establishment, and covariate components by reporting the corresponding coefficients on mean log value added from separate estimations. Models are estimated on the pooled sample. Only establishments with 1+ year of mean log value added are included, and the distribution of mean log value added is trimmed above 5. All regressions include a cubic polynomial in experience and a set of year dummies fully interacted with 5 education groups. Model 2 additionally includes dummies for 16 major industries and 11 federal states. Model 3 uses detailed three-digit industry codes (up to 254 dummies; some cells empty). Standard errors are clustered at the establishment level.
Source: LIAB Mover Model 9308

Table A.14: Gelbach's Decomposition of the Overall Gender Wage Gap Based on Pooled AKM Models

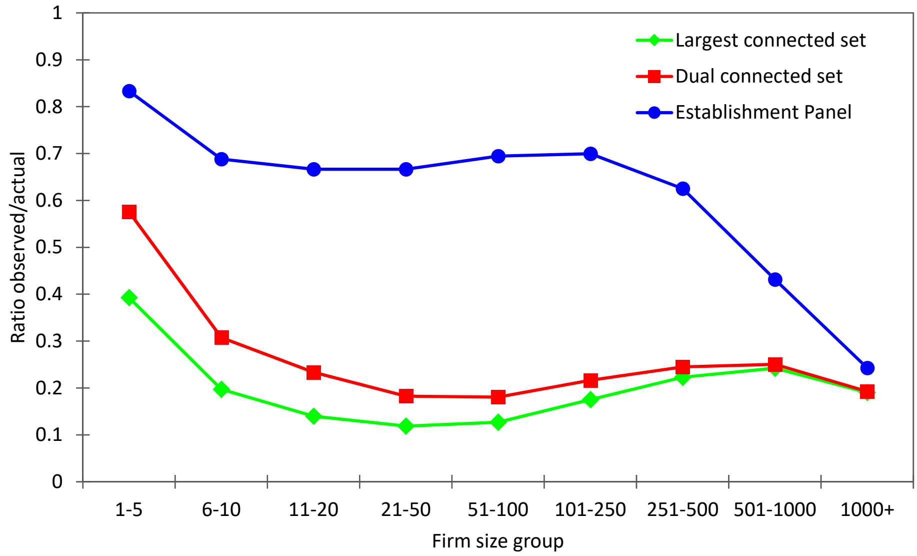
	1995-2001		2001-2008		Differences	
	All	Dual-connected	All	Dual-connected	All	Dual-connected
	(1)	(2)	(3)	(4)	(5)	(6)
Unadjusted gender wage gap	0.239	0.247	0.240	0.247	0.001	0.000
	(100.0)	(100.0)	(100.0)	(100.0)		
Share of component						
Firm-effects (ψ)	0.055	0.054	0.072	0.073	0.017	0.018
	(23.1)	(21.9)	(30.2)	(29.4)	(7.2)	(7.5)
Person-effects (α)	0.163	0.170	0.153	0.158	-0.010	-0.011
	(68.1)	(68.7)	(63.6)	(64.2)	(-4.3)	(-4.6)
Covariate index ($X\beta$)	0.021	0.023	0.015	0.016	-0.006	-0.007
	(8.8)	(9.3)	(6.2)	(6.4)	(-2.6)	(-2.9)
Observations						
# of person-year	14,964,310	13,049,037	16,805,826	14,677,435		
# of workers	2,744,864	2,526,929	2,769,838	2,544,888		
# of establishments	436,807	88,968	426,126	93,306		

Note: Sample in columns 1, 3, and 5 contains all workers in the largest connected set of the pooled model. Sample in remaining columns is restricted to firms in the dual-connected set, using the parameters of the pooled model. Entries are estimated using Gelbach's exact decomposition. The contribution of the covariate index ($X\beta$) is obtained by summing over all covariates for each worker, and regressing the compound heterogeneity index on the gender dummy (see Gelbach, 2016). Entries in parentheses are the percentage share of the raw gender gap attributable to the component indicated in the row heading. For ease of interpretation, I reversed the sign on the female dummy.
Source: LIAB Mover Model 9308

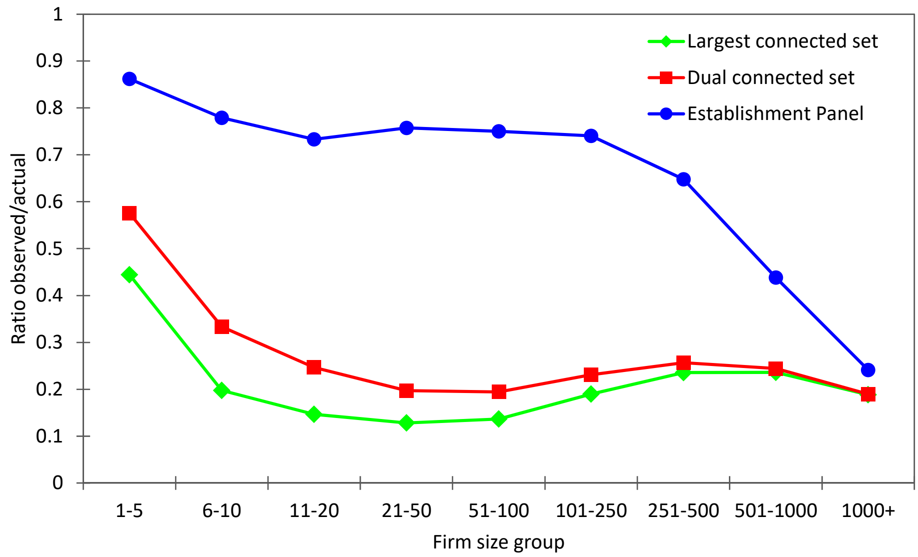
A.11 Appendix Figures

Figure A.1: Ratio of Observed to Actual Number of Full-time Workers (Male + Female) for Different Analysis Samples

A. 1995-2001

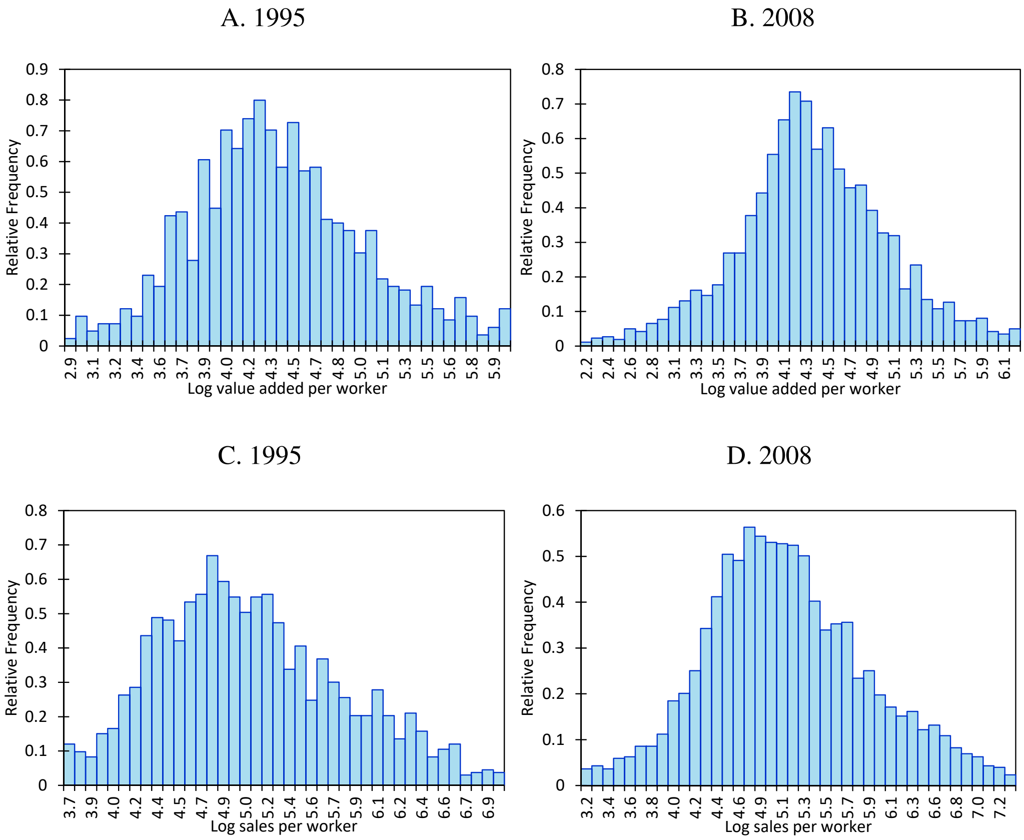


B. 2001-2008



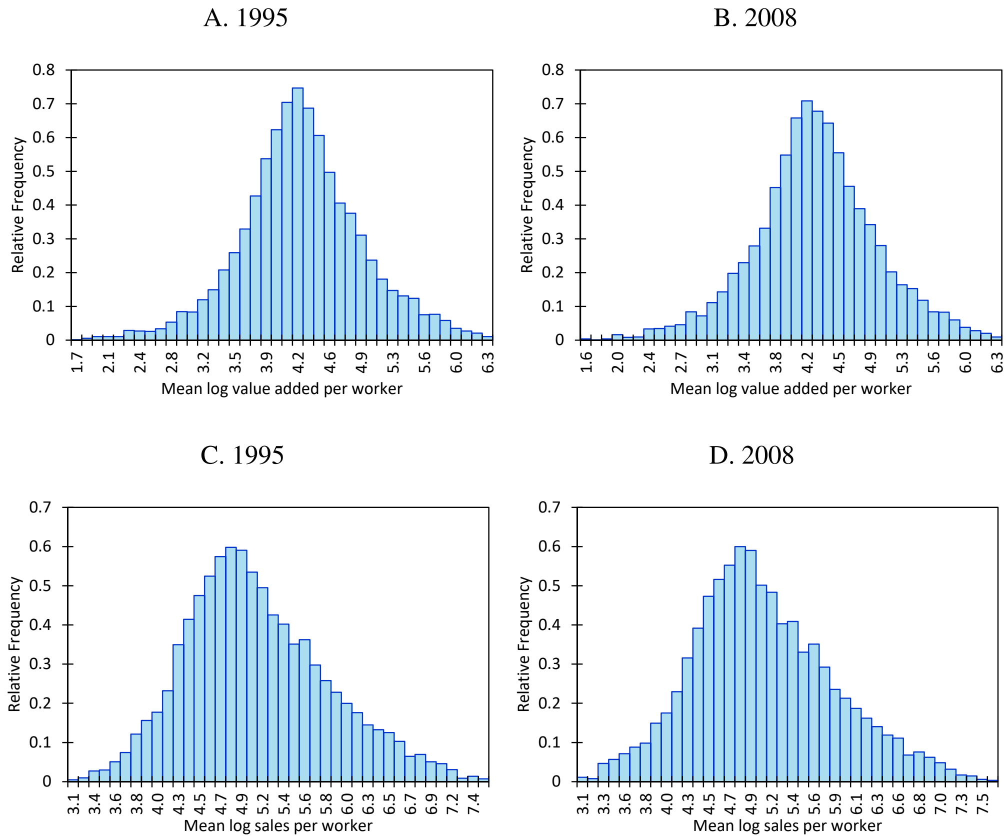
Note: Figure plots the shares of observed over actual number of full-time workers for three different analysis samples in two periods. The observed number of workers refers to the individuals that can be linked to an establishment via the establishment identifier. The actual number of workers refers to the figure recovered from the universe of employment biographies. The analysis samples are described in the main text. The unit of observation is the firm-year, and all employment refers to full-time workers.
Source: LIAB Mover Model 9308

Figure A.2: Histograms of Log Productivity per Worker in 1995 and 2008



Note: Figure shows histograms of log value added per worker (panels A and B) and log sales per worker (panels C and D) in 1995 and 2008. For construction of these variables, see main text and Appendix. Histograms weight each firm equally.
Source: LIAB Mover Model 9308

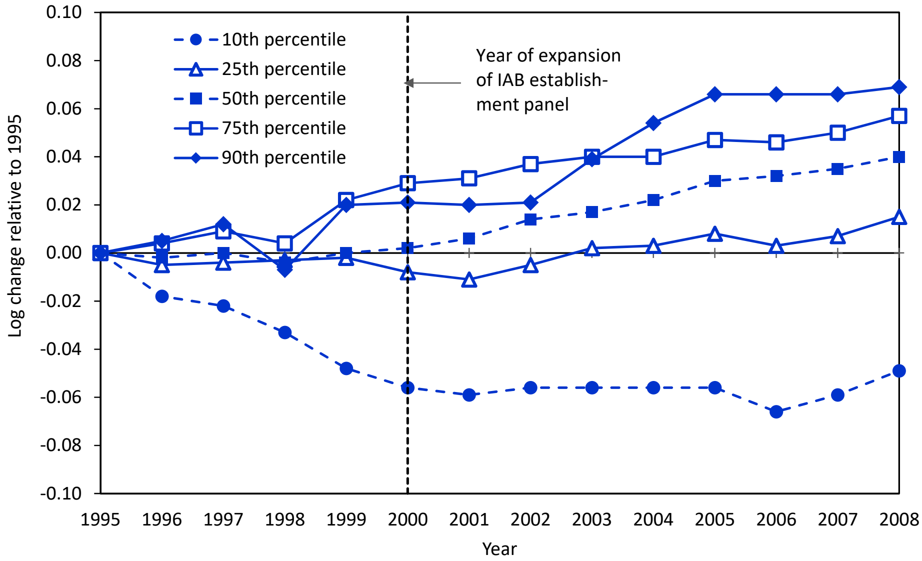
Figure A.3: Histograms of Mean Log Productivity per Worker in 1995 and 2008



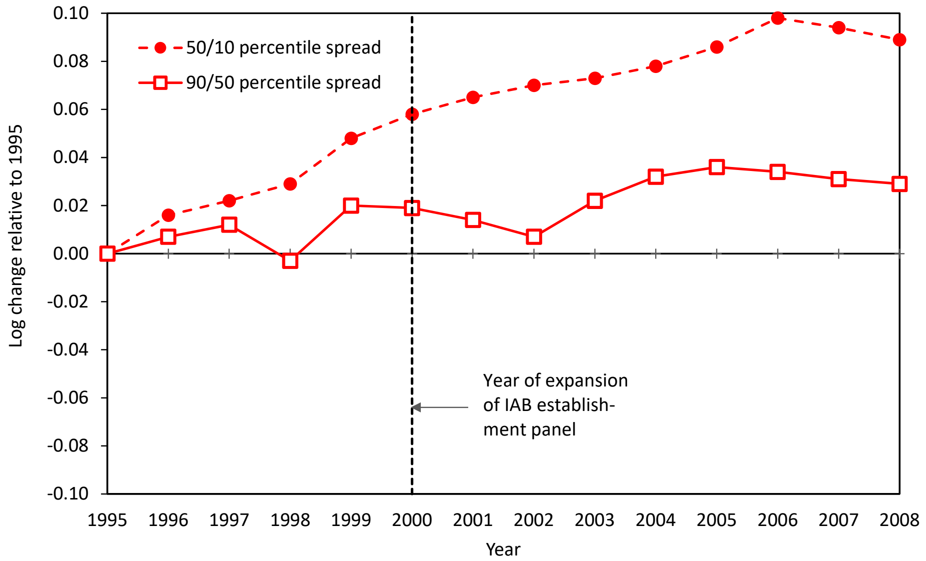
Note: Figure shows histograms of mean log value added per worker (panels A and B) and mean log sales per worker (panels C and D) in 1995 and 2008. For construction of these variables, see main text and Appendix. Histograms weight each firm equally.
Source: LIAB Mover Model 9308

Figure A.4: Percentile Growth of Mean Log Value Added Per Worker between 1995 and 2008

A. Evolution of Selected Percentiles of Mean Log Value Added per Worker



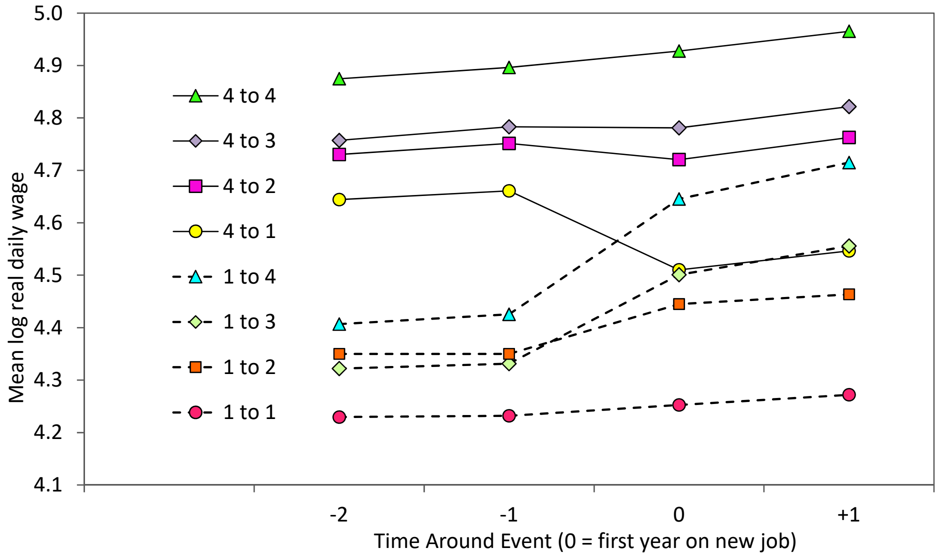
B. Evolution of Selected Percentile Gaps of Mean Log Value Added per Worker



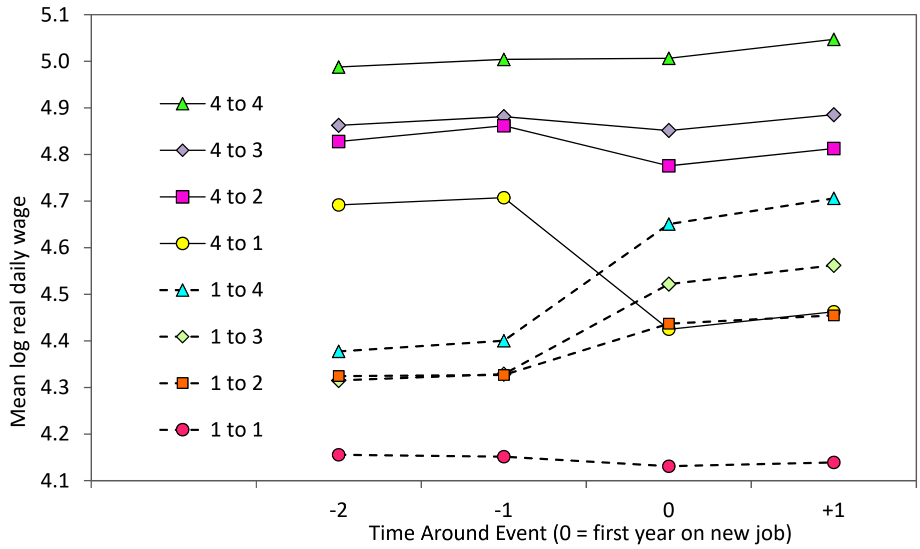
Note: Figure plots percentiles (panel A) and percentile spreads (panel B) of mean log value added over time. The distribution of productivity measures is based on establishments with at least one year of value added/sales (EP sample), and averaged across all years a firm is observed. All lines are weighted by person-years and scaled to 1995=0.
Source: LIAB Mover Model 9308

Figure A.5: Mean Wage Profiles of Male Movers Between Origin- and Destination Jobs Classified by Quartiles of Co-Worker Wages

A. 1995-2001



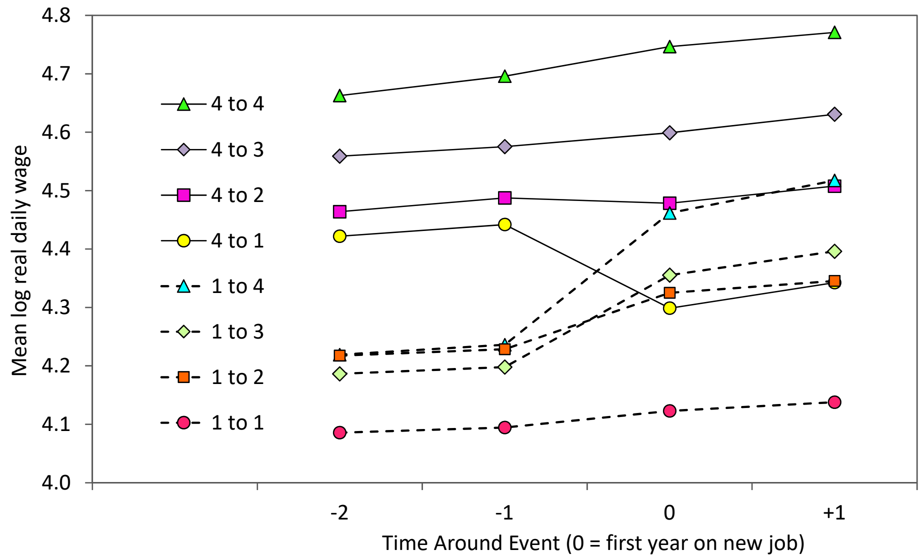
B. 2001-2008



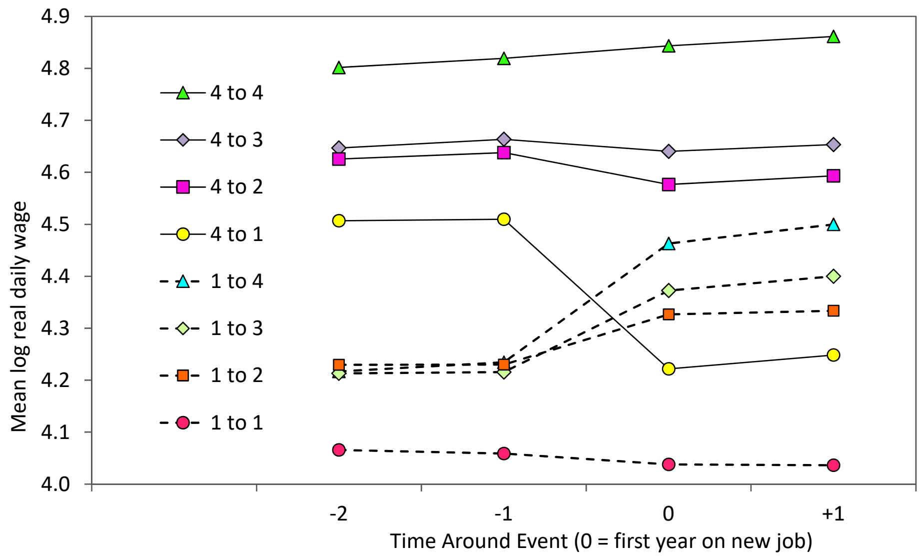
Note: Figure shows 4-year mean wage profiles of workers who move between jobs in the dual-connected set. Time 0 is the first year on the new job. Jobs are grouped into quartiles based on coworker wages in a given year. For a description of the event study, see Appendix.
Source: LIAB Mover Model 9308

Figure A.6: Mean Wage Profiles of Female Movers Between Origin- and Destination Jobs Classified by Quartiles of Co-Worker Wages

A. 1995-2001



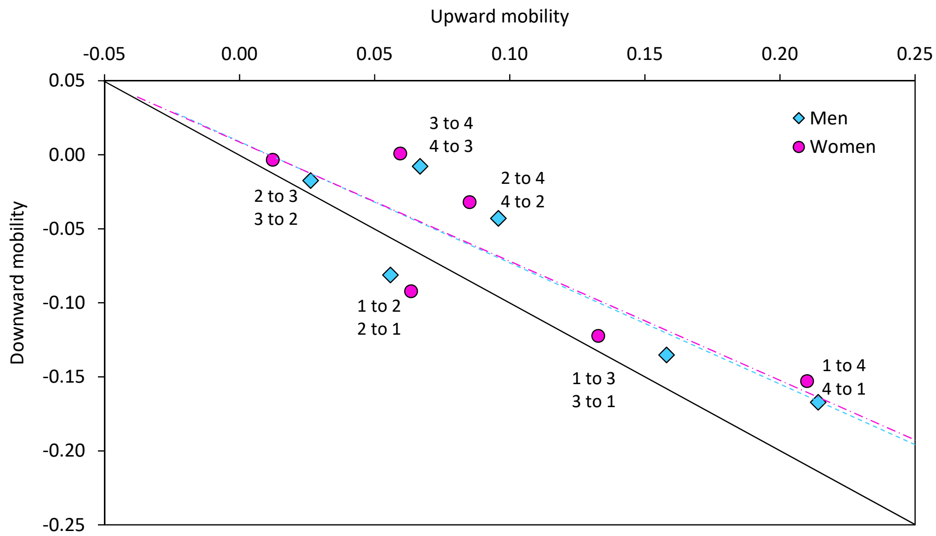
B. 2001-2008



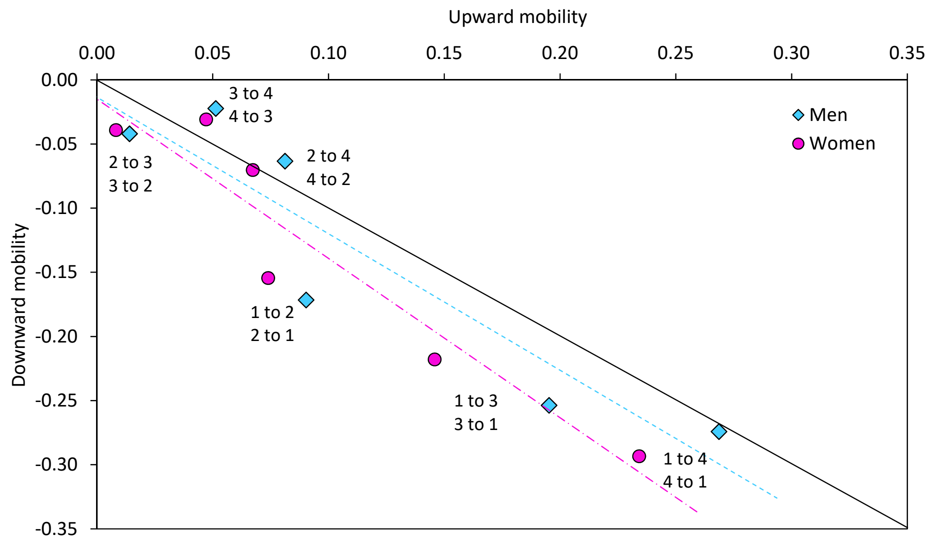
Note: See notes to Figure A.5.
Source: LIAB Mover Model 9308

Figure A.7: Regression-Adjusted Mean Wage Changes of Male and Female Movers Grouped into Co-Worker Wage Quartiles

A. 1995-2001



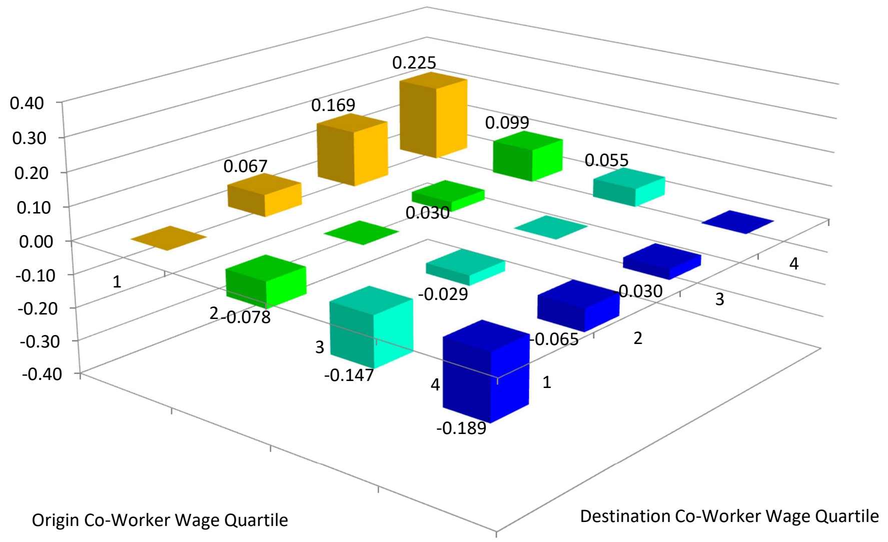
B. 2001-2008



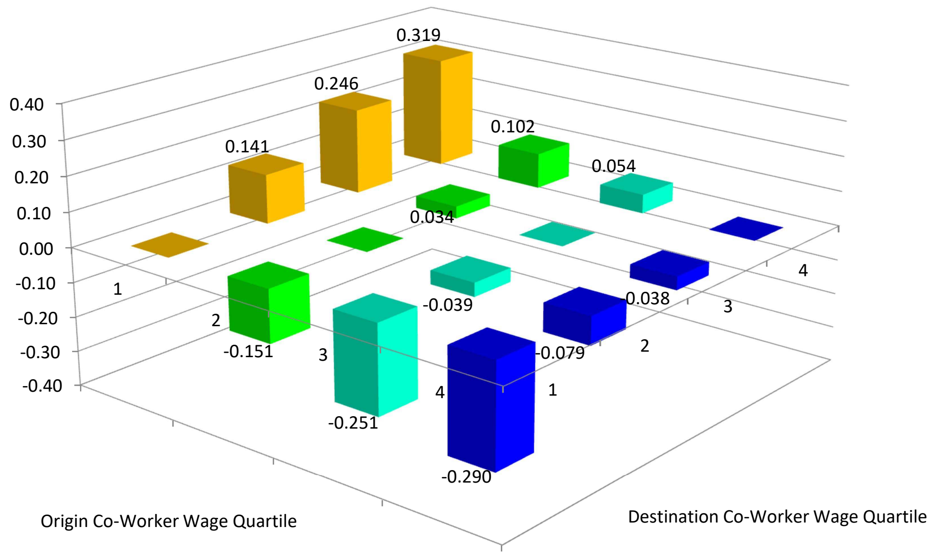
Note: Figure plots regression-adjusted average 3-year wage changes associated with downward and upward transitions for each gender. Points on the 45-degree line indicate perfect symmetry of upward and downward changes. The sample contains all firms in the dual-connected set.
Source: LIAB Mover Model 9308

Figure A.8: Regression- and Trend-Adjusted Mean Wage Changes of Men by Origin-Destination Quartiles

A. 1995-2001



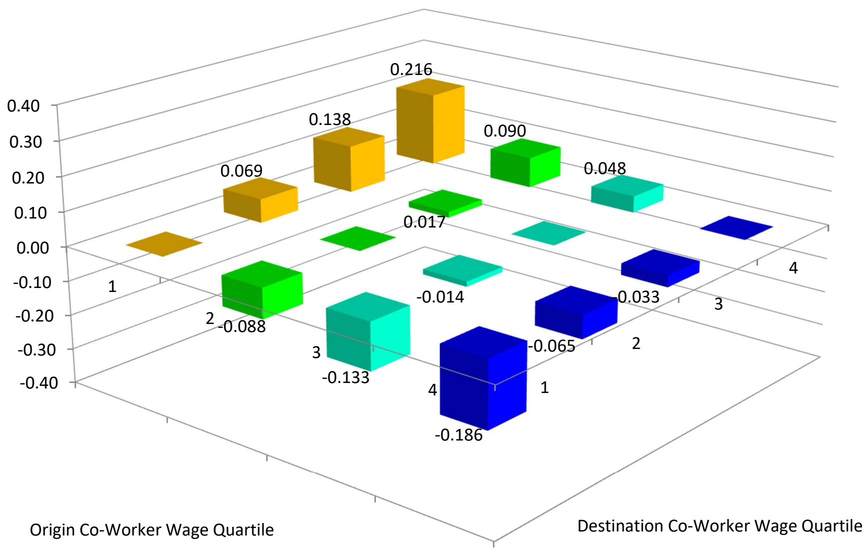
B. 2001-2008



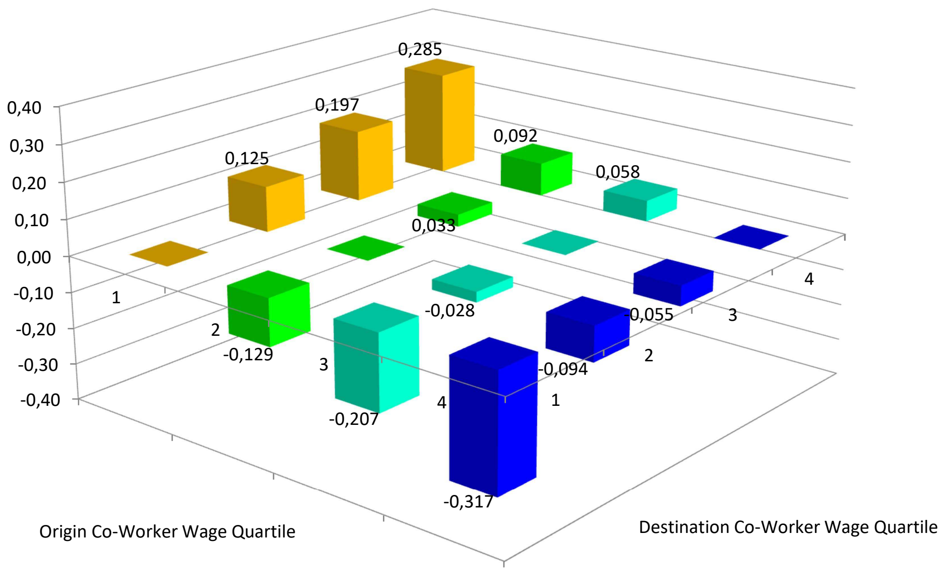
Note: Figure plots regression-adjusted 3-year wage changes of male movers between origin- and destination quartiles. In addition to a regression-adjustment, all values are trend-adjusted by subtracting the average 3-year wage change of workers who also move but stay within the same quartile.
Source: LIAB Mover Model 9308

Figure A.9: Regression- and Trend-Adjusted Mean Wage Changes of Women by Origin-Destination Quartiles

A. 1995-2001



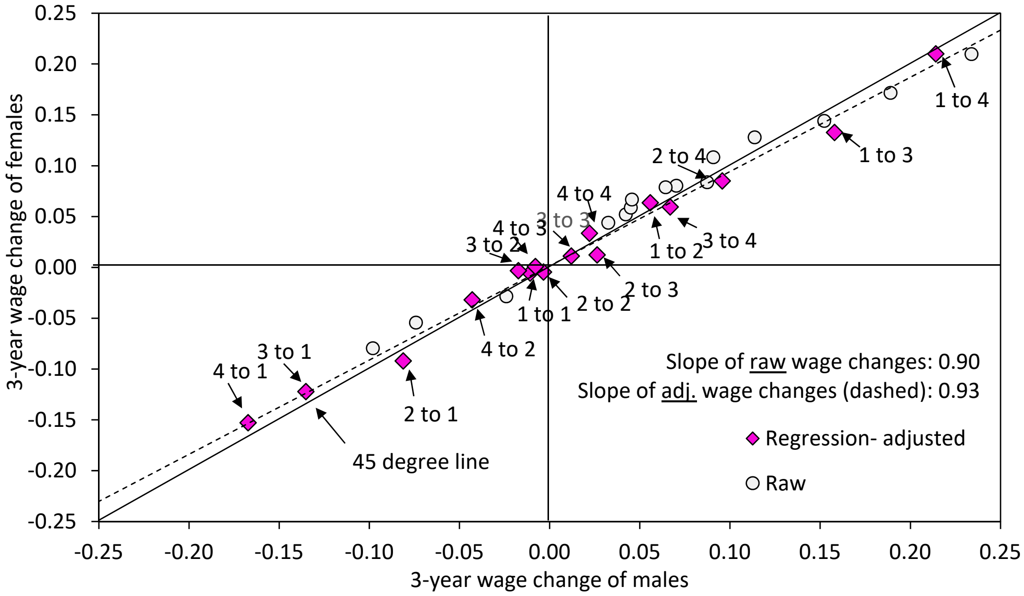
B. 2001-2008



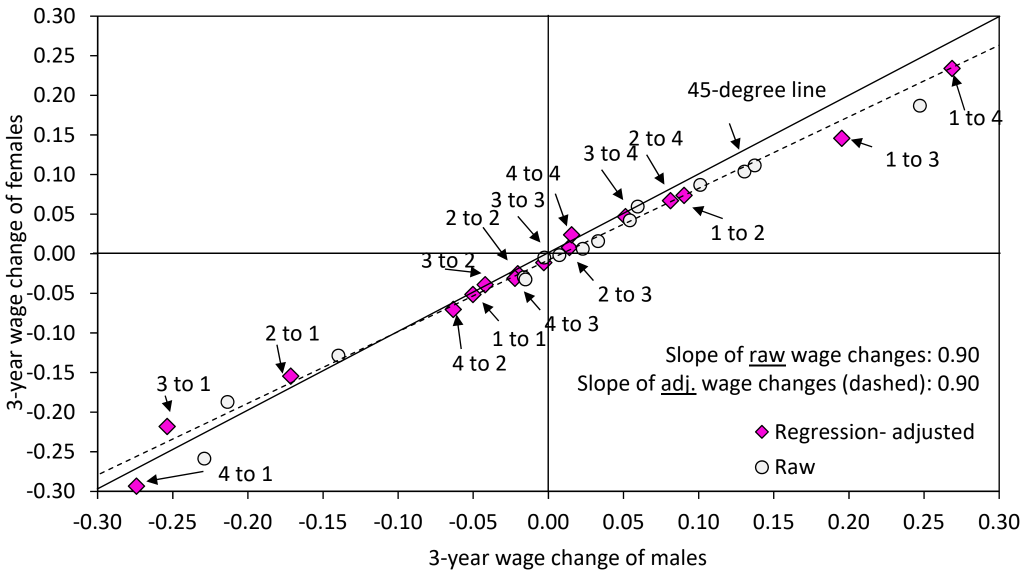
Note: Figure plots regression-adjusted 3-year wage changes of female movers between origin- and destination quartiles. In addition to a regression-adjustment, all values are trend-adjusted by subtracting the average 3-year wage change of workers who also move but stay within the same quartile.
Source: LIAB Mover Model 9308

Figure A.10: Raw and Regression-Adjusted Wage Changes of Female and Male Workers Moving Between Same Origin and Destination Quartiles

A. 1995-2001



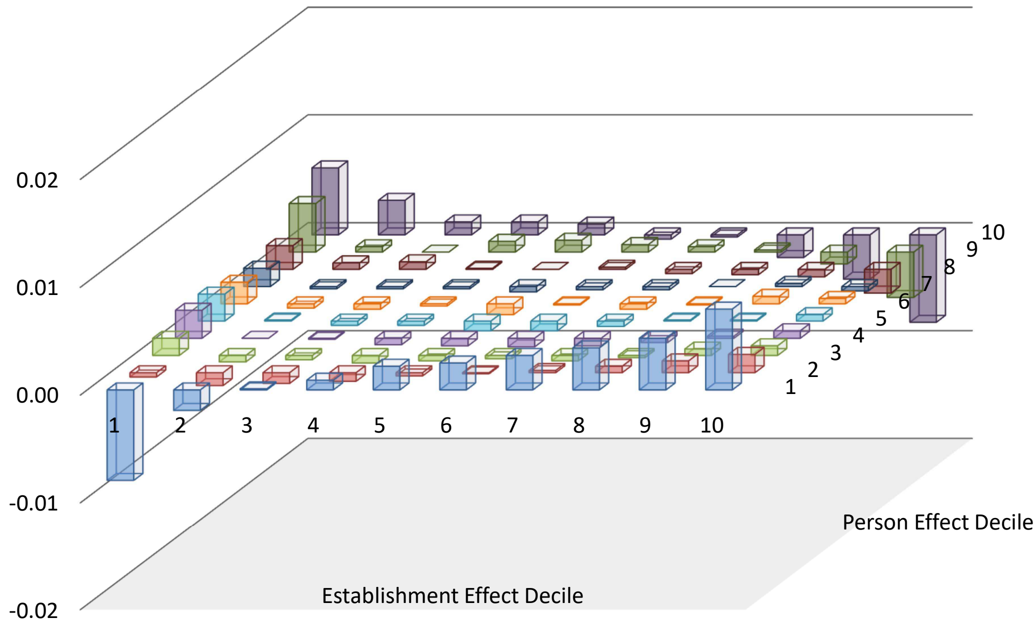
B. 2001-2008



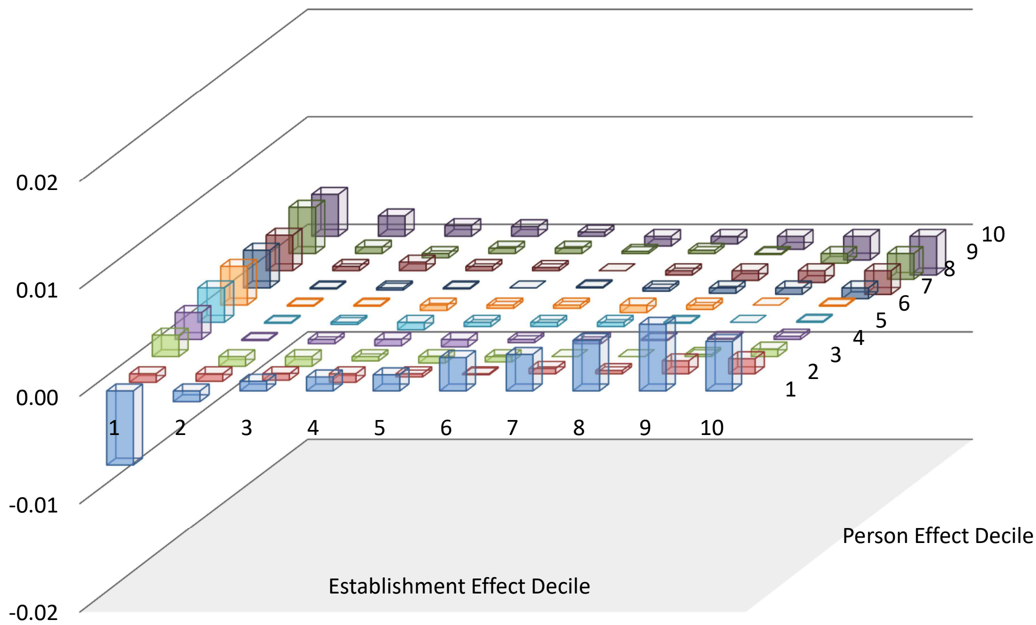
Note: Figure plots raw and regression-adjusted 3-year female mean wage changes associated with job transitions between co-worker wage quartiles against corresponding male wage changes. Results based on the dual-connected set. The dashed line is a linear regression of female changes on male changes. The solid line is a 45-degree line. One outlier of raw wage changes not shown.
Source: LIAB Mover Model 9308

Figure A.11: Mean AKM Residuals Across Deciles of Person and Firm Effects: Men

A. 1995-2001



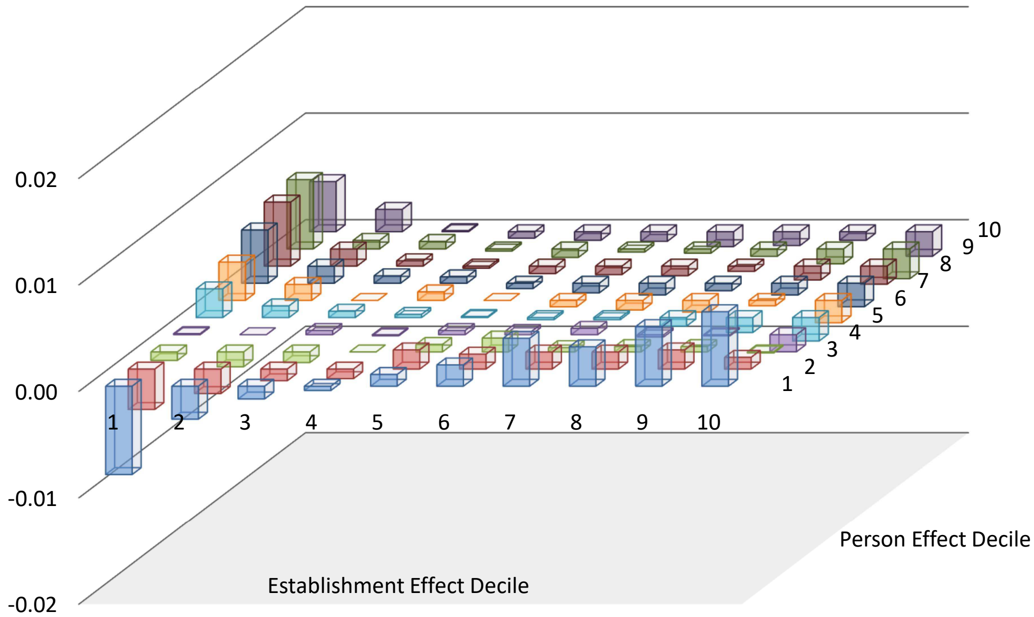
B. 2001-2008



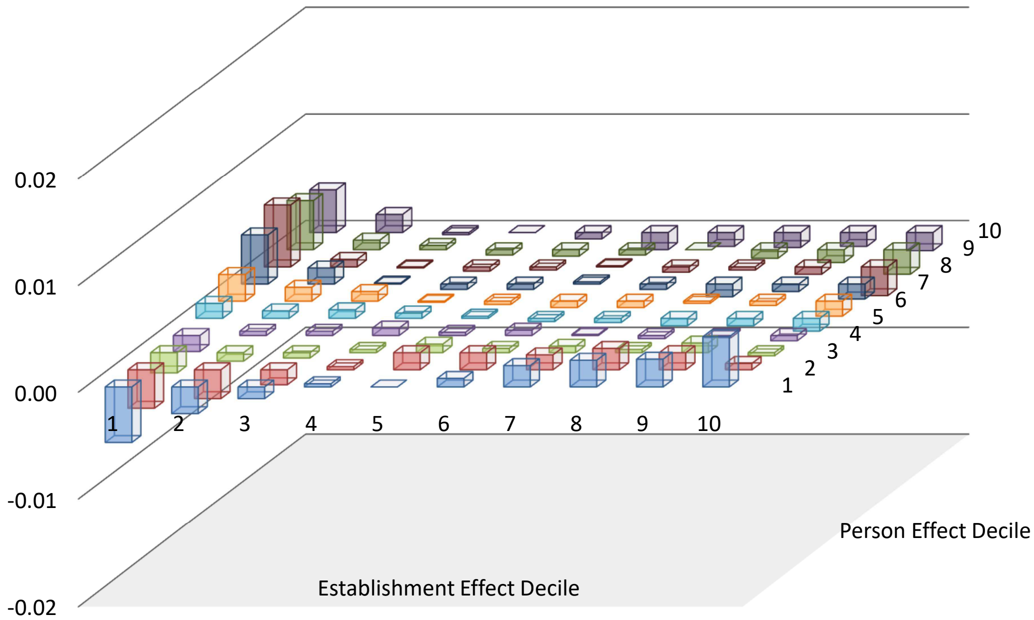
Note: Figure shows mean AKM residuals across 100 cells of person and firm effect interactions (10 deciles of person effects interacted with 10 deciles of firm effects). The corresponding AKM models are summarised in Table 2.2.
Source: LIAB Mover Model 9308

Figure A.12: Mean AKM Residuals Across Deciles of Person and Firm Effects: Women

A. 1995-2001

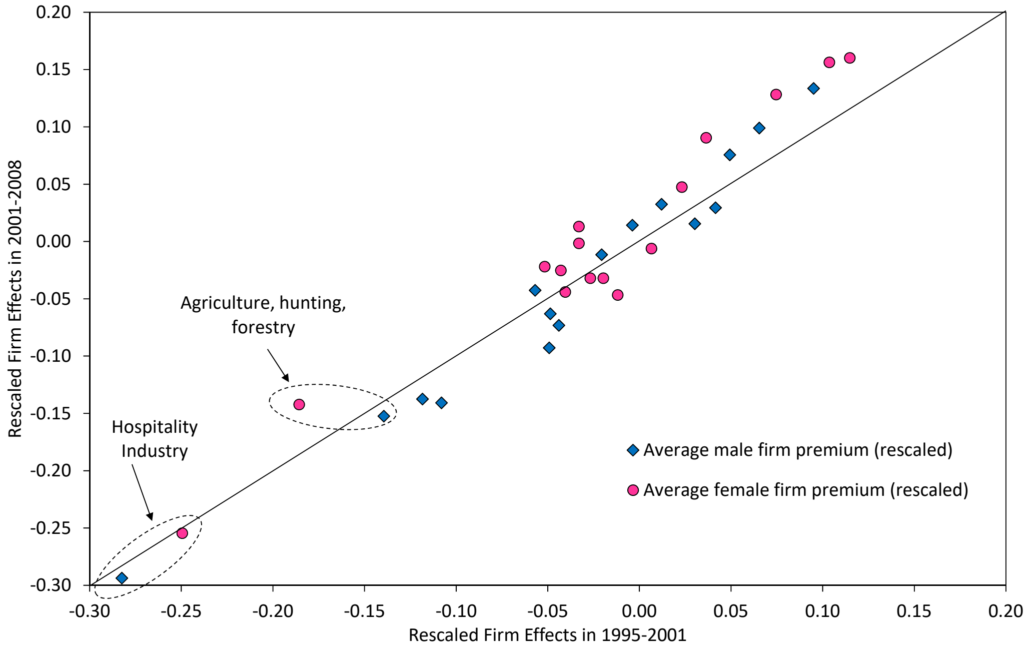


B. 2001-2008



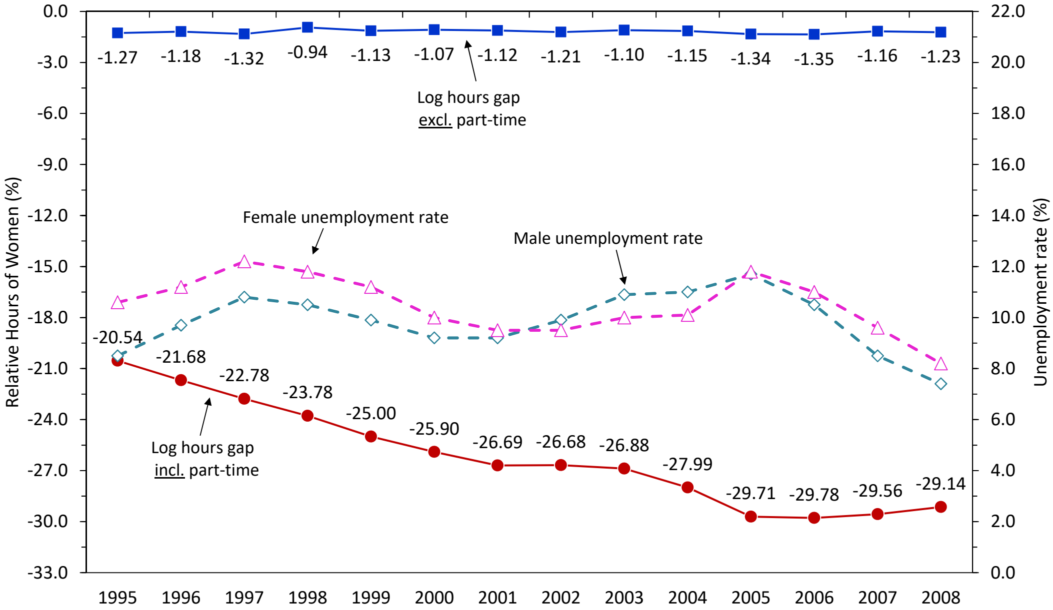
Note: Figure shows mean AKM residuals across 100 cells of person and firm effect interactions (10 deciles of person effects interacted with 10 deciles of firm effects). The corresponding AKM models are summarised in Table 2.2.
Source: LIAB Mover Model 9308

Figure A.13: Estimated Male and Female Firm Wage Premiums Across 16 Major Industry Groups in 1995-2001 and 2001-2008



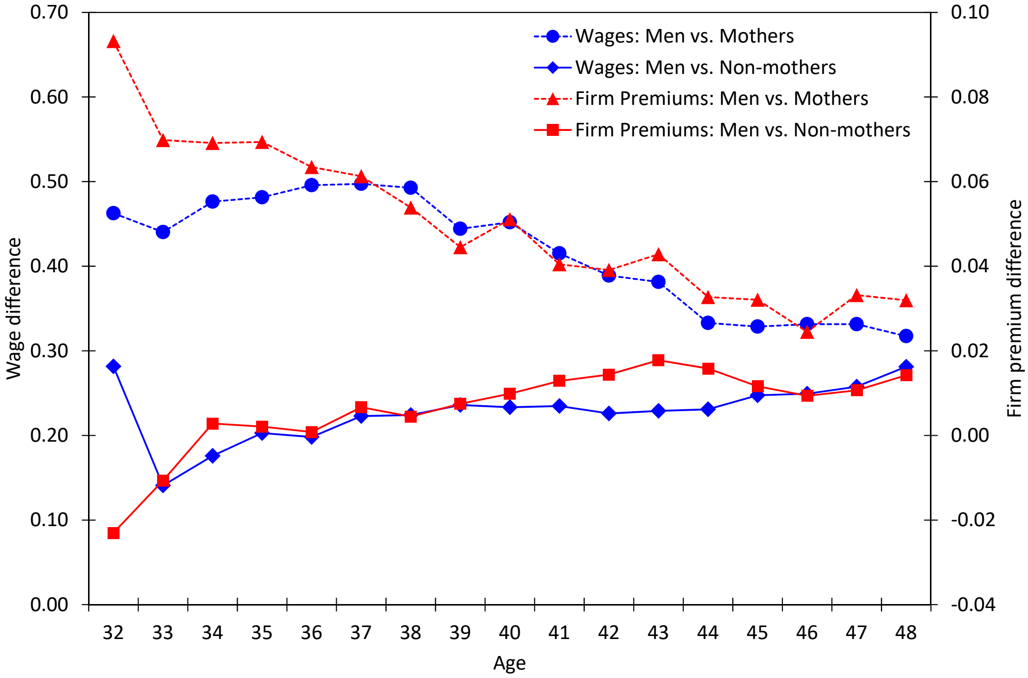
Note: Figure plots average estimated firm effects of men and women within 16 industry groups in 2001-2008 against the corresponding values in 1995-2001. Results based on dual-connected sample in each period. For the purpose of comparability, the gender-specific firm effects are rescaled so that they sum to zero for each gender in each period.
Source: LIAB Mover Model 9308

Figure A.14: Average Usual Weekly Working Hours of Full-Time Men and Women Between 1995 and 2008



Note: Plot shows the evolution of relative weekly working hours of women. Calculations are based on the OECD concept of usual weekly working hours of the working age population between 15 and 64. Data on unemployment rates refers to the total civilian labour force. Gender-specific unemployment rates refer to the same age groups.
Source: LIAB Mover Model 9308; OECD.StatExtract data base, accessed on 17th April 2016; Federal Statistical Office

Figure A.15: Age Profiles of the Men/Mother and Men/Non-mother Wage and Firm Rent Differentials



Note: Figure shows the mean wage/firm premium gap between men and mothers (dashed) and men and non-mothers (solid) at ages 32 to 48. At each age, I select mothers who gave birth 10 years earlier. Ages 30/31 and 49/50 are omitted due to very small samples of mothers.
Source: LIAB Mover Model 9308

B Appendix to Chapter 3: The Impact of Immigrants on Native Wages and Employment: An Analysis of Refugee Inflows in the Early 1990s

B.1 Sample Processing

B.1.1 Sample of Integrated Employment Biographies (SIAB) 1975-2010

Our analysis is based on the Sample of Integrated Labour Market Biographies (SIAB) 1975-2010 from the German Institute of Employment Research (IAB). For a detailed description, see vom Berge et al. (2013) The sample provides individual level administrative data for a 2% sample of employees liable to social security contributions. From the initial sample, we delete workers in training ($stib = 0$), workers from home ($stib = 7$), employees in partial retirement ($erwstat = 103$), interns and student trainees ($erwstat = 105, 106$), and individuals with missing employment information. Marginally employed ($erwstat = 109, 209$) are not considered since they are not consistently observed prior to 1999. For each individual, we delete parallel employment spells ($level2 \neq 0$), and restrict the sample to worker spells covering June 30th each year. We consider the remaining sample as the labour force which is composed of employed persons ($erwstat \geq 101$), with part-time employees weighted by 1/2 ($stib = 8$) or 2/3 ($stib = 9$), and unemployed ($erwstat \leq 5$) persons.

Since the data do not record the place-of-birth, we follow Bonin (2005), D'Amuri et al. (2010), and Glitz and Wissmann (2017) using citizenship information to identify natives and immigrants. We impute missing or inconsistent values for nationality following the procedure of Drews et al. (2007). After filling gaps, we impute a constant citizenship at the person level assigning the modal value to all years; in case of ties, we assign German nationality. We regard individuals whose first employment spell is in East Germany as East German migrants. We note that since we only observe employment spells in East Germany after 1991, this method allows us to only partially identify East German migrants. For the identification of ethnic Germans (so-called Spätaussiedler who received German citizenship upon arrival), we follow Brücker and Jahn (2011), exploiting information on the receipt of any type of subsidy exclusively offered to ethnic Germans to support their integration (e.g., language courses; $lart = 1010, 1016, 1018, 1036, 1037, 1041, 1045, 1058$).

We impute missing or unknown values for education with an individual's information in the previous spell, if available, and impose that individuals cannot downgrade education. We group education levels into two skill groups: Unskilled are individuals with at most a high school degree (Abitur; $bild = 1, 3$), including individuals with missing information after the correction (2.9% of observations). This choice is guided by comparing average wages across education groups between 1985 and 1995. Skilled are individuals who completed an apprenticeship training or obtained a tertiary degree (e.g., Bachelor, PhD; $bild = 2, 4, 5, 6$). We further impute missing or unknown regional information for employed and unemployed with the most recent information of the previous or next spell, if available.

For our wage analysis, we only keep full-time workers ($stib \neq 8, 9$) and deflate wages by the German Consumer Price Index (CPI), with 1995 as the base year. We consider wages that are above the annual social security contribution limits provided by the IAB (rounded to the nearest lower integer) as right censored. We then impute censored wages following the approach in Glitz (2012), which fits a series of annual Tobit models of the log wage on a dummy for immigrants, seven education groups (including missing and no education as separate categories), gender, and the commuting zone. We then replace each censored wage observation by an uncensored prediction based on the estimated parameters and a random draw from the associated truncated normal distribution. This imputation approach is by now common practice for this data and has been extensively evaluated (e.g., Dustmann et al., 2009; Glitz and Wissmann, 2017).

B.1.2 Other Data Sources

For our instrument, we determine the shortest airline distance between the border of each local labour market and the eastern and southern border of West Germany using geo-spatial data from the Federal Office for Cartography and Geodesy.

To determine the share of immigrants in the population on the regional level in 1961, we use data from the 1961 Census provided by the Genesis Data Archive. The Stata file is available online at GESIS data archive, file name ZA2472. Bavaria and North Rhine-Westphalia are not included.

In addition, we use district level population data from the Federal Statistical Office for years 1985-2001. For years 1985-1989, we converted scanned versions of the Statistical Yearbooks for Germany (available online) into machine-readable data sets. Since our analysis refers to June 30th each year, while the Statistical Yearbooks are dated to January 1st, we use the arithmetic mean between two consecutive years as the population measure in our analysis.

We obtain task information on the occupational level from the Qualification and Career Survey (QCS) conducted by German Federal Institute for Vocational Training (Bundesinstitut für Berufsbildung, BIBB). We use the 1985 wave and include all workers between 18 and 64 who work between 30 to 90 hours. We distinguish between simple and advanced occupations based on the task composition associated with each job. As in Prantl and Spitz-Oener (2014), we classify the following tasks as “advance”: designing, making plans, restoring, servicing machines and equipping machines, and define “job complexity” as the average share of advanced tasks in an occupation.[1] We consider an occupation as advanced (simple) if the associated share is above the employment weighted median of the job complexity index.

B.2 Regulatory Framework

During our analysis period (1988-1993), numerous amendments to the laws regulating the access of asylum seekers to the West German labour market took place. In general, German law granted unrestricted access to the labour market to any asylum seeker conditional on a favourable asylum decision from the Federal Office for Migration and Refugees (*Bundesamt für Migration und Flüchtlinge*).[2] However, prior to an asylum decision, access to the labour market was restricted by waiting periods. With the introduction of the new Asylum law on January 15th, 1987, asylum seekers were not allowed to work for a period of five years unless they came from former Eastern Bloc countries in which case the waiting period was limited to 12 months (*Gesetz zur Änderung asylverfahrensrechtlicher, arbeitserlaubnisrechtlicher und ausländerrechtlicher Vorschriften*) (Münch, 1992; Thränhardt, 2015) It was not until 1990/1991, when the waiting period for all asylum seekers was first harmonized to 12 months on December 21st, 1990 and eventually banned on June 21st, 1991 (*Neunte Verordnung zur Änderung der Arbeitserlaubnisverordnung*). On April 1st, 1993, a revision of the asylum procedure reintroduced a waiting period for asylum seekers while residing in reception centres. The residence of asylum seekers in these centres was mandatory and lasted from a minimum of six weeks to a maximum of three months. Only three months later, on July 1st, 1993, a fundamental renovation of the asylum law became effective, which introduced the principle of safe countries of origin and guidelines on third countries (which facilitated a repatriation of refugees to the EU border countries). These renovations led to a drastic reduction of immigration inflows to West Germany (Münch, 1992).

B.3 Displacement Effect

In this section, we show how to interpret the employment coefficient from our main regression in different subpopulations. We focus on two aspects that impact on the magnitude of the coefficient: first, the share of the subpopulation in total employment; and second, the share of immigrants.

Let us denote by $N_{ir,t}$ the number of employed natives in group i, region r, and year t. The total number of employed natives is given by $N_{r,t} = \sum_i N_{ir,t}$ and the total number of

[1]The occupation variable contains 120 occupational categories, which are aggregates of 330 occupations from the German *Klassifikation der Berufe 1988* classification used in the SIAB data.

[2]Before 2005, this institution was called Federal Office for the Recognition of Foreign Refugees (*Bundesamt für die Anerkennung ausländischer Flüchtlinge*).

immigrants is given by $I_{r,t}$. A simplified version of our regression reads as follows:

$$\frac{\Delta N_{ir,t}}{N_{ir,t-1}} = \delta_i \frac{\Delta I_{rt}}{N_{r,t-1} + I_{r,t-1}} \tag{B.1}$$

The left hand side represents the percentage change in employment of native skill group i between $t-1$ and t, and the right hand side represents the percentage inflow of all immigrants in total employment over the same time period. In our empirical analysis, we estimate eq. (B.1) for various i, holding the right hand side constant. Rearranging a little bit yields the following:

$$\Delta N_{ir,t} = \delta_i \Delta I_{rt} \frac{N_{ir,t-1}}{N_{r,t-1} + I_{r,t-1}} \tag{B.2}$$

This expression represents the impact of immigrants on a per worker basis. For example, for $\delta_i = -1$, we have that, as one immigrant enters employment in a region ($\Delta I_{rt} = 1$) between $t-1$ and t, the number of native workers in group i who are displaced (or nor longer enter employment) is given by the $t-1$ share of that group in total employment (natives+immigrants). Put differently, if the share of native unskilled in total employment is about 15%, then a coefficient of -1 implies that 0.15 native unskilled workers are displaced. Conversely, if more than 0.15 unskilled are displaced, the coefficient is more negative. Note that in the case of the pooled sample, the share is just equal to the employment share of natives.

To illustrate, consider the following numerical example. Let $N_{r,t-1} = 90$, $I_{r,t-1} = 10$, $N_{Lr,t-1} = 18$ and $N_{Hr,t-1} = 72$. These figures imply an unskilled share among natives of about 20%. Substituting these numbers into eq. (B.2) gives:

$$\Delta N_{ir,t} = \delta_i \Delta I_{r,t} \frac{18}{72+18+10} = \delta_i \Delta I_{r,t} \frac{18}{100} \tag{B.3}$$

Now, suppose that for every 10 immigrants finding employment in region r, 5 unskilled natives are displaced (into another region or into nonemployment), i.e. $\Delta I_{r,t} = 10$ and $\Delta N_{ir,t} = -5$. Then:

$$-5 = \delta_i \times 10 \times \frac{18}{100} \Leftrightarrow \delta_i = -2.78 \tag{B.4}$$

As indicated above, this numerical example shows that if certain groups exhibit employment losses larger than their employment share, this leads to coefficients larger than -1 in absolute values.

Now, consider the second issue, the share of immigrants. Using the equations from above, it is obvious that a larger number of immigrants in the initial period also increases the magnitude of the displacement coefficient. To illustrate, assume that $I_{r,t-1} = 0$, i.e., there are no immigrants in the base period. Then

$$\Delta N_{ir,t} = \delta_i \Delta I_{r,t} \frac{18}{72+18} = \delta_i \Delta I_{r,t} \frac{18}{90} \tag{B.5}$$

The same shock of $\Delta I_{rt} = 10$ and the same displacement of $\Delta N_{ir,t} = -5$ then implies:

$$-5 = \delta_i \times 10 \times \frac{18}{90} \Leftrightarrow \delta_i = -2.5 \tag{B.6}$$

which is exactly 90% (the employment share of natives) the size of the coefficient from an estimation that includes immigrants in the denominator.

B.4 Modelling Framework: An Equilibrium Model with Heterogeneous Labour Supply and Wage Rigidities

This appendix provides the basic modelling set-up along with all necessary calculations to derive the estimation equations relating native wage and employment responses to an immigrant-induced labour supply shock. While the basic model has been featured in many migration studies, the extensions we consider here — in particular, heterogeneous labour supply — were first introduced by Dustmann et al. (2017). In the following, we stick to their notation, but provide all additional steps required to arrive at the final equations. Throughout the following derivations, we focus on a single representative local labour market, and omit an area-subscript. Each local labour market is considered small, in that wage and employment adjustments in other areas do not affect the equilibrium outcome in the labour market under consideration (and vice versa). While an approximation, it is likely to be consistent with our empirical specification, where a region corresponds to the commuting zone.

B.4.1 Production

Assume that output Q is produced according to a Cobb-Douglas production function:

$$Q = AK^{\alpha}L^{1-\alpha} \tag{B.7}$$

where K denotes capital and L a CES aggregator of unskilled and skilled labour, g={U,S}

$$L = \left[\theta_U L_U^{\beta} + \theta_S L_S^{\beta}\right]^{\frac{1}{\beta}} \tag{B.8}$$

with $\theta_U + \theta_S = 1$, and $\sigma = \frac{1}{1-\beta}$ denoting the elasticity of substitution between the two skill groups ($\beta \leq 1$). We assume that natives (L_g^N) and immigrants (L_g^I) are perfect substitutes within each skill group, i.e., $L_g = L_g^N + L_g^I$. In our empirical specification, we do not attempt to assign immigrants to skill groups, so that the question whether immigrants are perfect or imperfect substitutes for natives will be part of the parameter that we estimate (in the model, it would show up as another nest within each skill group).[3]

[3] It would make a difference, if we were to give the parameters of the model a structural interpretation, since then the assumption of perfect substitutability might bias the estimates of, e.g., the elasticity of substitution

B.4.2 Factor Demands

We begin by deriving the labour demand function. With firms being price takers on the product, labour, and capital markets (we normalise the price of the output good to unity), the optimal choice of labour and capital requires that marginal costs equal marginal products. For labour L_g, we obtain:

$$\begin{aligned}
\frac{\partial Q}{\partial L_g} &= A(1-\alpha)L^{-\alpha}K^{\alpha}\frac{1}{\beta}\left[\theta_U L_U^{\beta}+\theta_S L_S^{\beta}\right]^{\frac{1-\beta}{\beta}}\theta_g\beta L_g^{\beta-1} \text{ for g=U, S}\\
&= A(1-\alpha)L^{-\alpha}K^{\alpha}L^{1-\beta}\theta_g L_g^{\beta-1} \stackrel{!}{=} w_g\\
\Rightarrow \log w_g &= \log(A(1-\alpha))+\alpha(\log K-\log L)+(\beta-1)(\log L_g-\log L)+\log\theta_g \quad \text{(B.9)}
\end{aligned}$$

and for capital K, we get:

$$\begin{aligned}
\frac{\partial Q}{\partial K} &= AL^{1-\alpha}\alpha K^{\alpha-1} \stackrel{!}{=} r\\
\Rightarrow \log r &= \log(\alpha A)+(\alpha-1)(\log K-\log L) \qquad \text{(B.10)}
\end{aligned}$$

Suppose that the local supply of capital depends on the local rental rate of capital, r, and the rental rates of capital in all other regions, **r'**, where **r'** is a vector. That is

$$K = h(r,\mathbf{r'}), \qquad \text{(B.11)}$$

which implies an (own-)elasticity of capital supply

$$\frac{\partial K}{\partial r} = \frac{\partial h(r,\mathbf{r'})}{\partial r}\frac{r}{h(r,\mathbf{r'})} = \frac{1}{\lambda}. \qquad \text{(B.12)}$$

Here, λ denotes the inverse elasticity, i.e., the percentage change of the rental rate r for a 1% change in the local capital stock.

In the next steps, we will totally differentiate the FOC's to derive the equilibrium response in terms of changes. We begin with the demand for capital, and then move on to the demand for labour. First, rewrite the capital supply elasticity in terms of logarithms:

$$\frac{\partial K}{\partial r}\frac{r}{K} = \frac{1}{\lambda} \Leftrightarrow \frac{\frac{\partial K}{K}}{\frac{\partial r}{r}}\frac{r}{K} = \frac{1}{\lambda} \Leftrightarrow \frac{d\log K}{d\log r} = \frac{1}{\lambda} \Leftrightarrow d\log r = \lambda d\log K \qquad \text{(B.13)}$$

between skill groups.

Next, totally differentiate the FOC of capital demand:

$$
\begin{aligned}
\log r &= \log(\alpha A) + (\alpha - 1)(\log K - \log L) \\
\Rightarrow d\log r &= (\alpha - 1)(d\log K - d\log L) \\
\Leftrightarrow \lambda d\log K &= (\alpha - 1)(d\log K - d\log L) \\
\Leftrightarrow d\log K(\lambda + 1 - \alpha) &= (1 - \alpha)\, d\log L \\
\Leftrightarrow d\log K &= -\frac{\alpha - 1}{1 - \alpha + \lambda} d\log L
\end{aligned} \tag{B.14}
$$

The third line follows from substitution of (B.13).

B.4.3 Equilibrium

We start by deriving a firm's change in the demand for *native* workers (net of immigrant workers). In what follows, we assume (without loss of generality) that there are no immigrants in the baseline period. First, we note that total skill-specific employment is given by $L_g = L_g^N + L_g^I$ and total employment is given by $L = L_U + L_S$. In the baseline period, we have $L_g = L_g^N$ for g=U, S.

$$
\begin{aligned}
dL_g = dL_g^N + dL_g^I \Leftrightarrow \frac{dL_g}{L_g} = \frac{dL_g^N}{L_g} + \frac{dL_g^I}{L_g} \Leftrightarrow \frac{dL_g}{L_g} &= \frac{dL_g^N}{L_g^N} + \frac{dL_g^I}{L_g^N} \\
\Leftrightarrow d\log L_g &= \frac{dL_g^N}{L_g^N} + \frac{\pi_g^I}{\pi_g^N} \underbrace{\frac{dL_g^I}{L^N}}_{=dI}
\end{aligned} \tag{B.15}
$$

Here, we used the fact that we can express the total skill supply as $L_g = \pi_g^N L^N$ and $dL_g^I = \pi_g^I dL^I$ with $\pi_g^N = \frac{L_g^N}{L_U^N + L_S^N}$ and $\pi_g^N = \frac{L_g^I}{L_U^I + L_S^I}$, i.e., as fractions of the total supply of each nationality.

We next differentiate the CES aggregator of unskilled and skilled labour:

$$
\begin{aligned}
L(L_U, L_S) &= \left[\theta_U L_U^\beta + \theta_S L_S^\beta\right]^{\frac{1}{\beta}} \\
\Rightarrow dL &= \frac{\partial L(L_U, L_S)}{\partial L_U} dL_U + \frac{\partial L(L_U, L_S)}{\partial L_S} dL_S \\
&= \frac{1}{\beta}\left[\theta_U L_U^\beta + \theta_S L_S^\beta\right]^{\frac{1-\beta}{\beta}} \theta_U \beta L_U^{\beta-1} dL_U + \frac{1}{\beta}\left[\theta_U L_U^\beta + \theta_S L_S^\beta\right]^{\frac{1-\beta}{\beta}} \theta_S \beta L_S^{\beta-1} dL_S \\
&= L^{1-\beta}\theta_U L_U^{\beta-1} dL_U + L^{1-\beta}\theta_S L_S^{\beta-1} dL_S \\
\Leftrightarrow \frac{dL}{L} &= L^{-\beta}\theta_U L_U^{\beta-1} dL_U + L^{-\beta}\theta_S L_S^{\beta-1} dL_S \\
&= \underbrace{\frac{\theta_U L_U^\beta}{\theta_U L_U^\beta + \theta_S L_S^\beta}}_{=s_U} \frac{dL_U}{L_U} + \underbrace{\frac{\theta_S L_S^\beta}{\theta_U L_U^\beta + \theta_S L_S^\beta}}_{=s_S} \frac{dL_S}{L_S} \\
\Leftrightarrow d\log L &= s_U d\log L_U + s_S d\log L_S
\end{aligned} \tag{B.16}
$$

Note that $s_U + s_S = 1$, which we will use below. The last row expresses the percentage change in total labour inputs as an efficiency-weighted average of the percentage changes of each labour type. We can now substitute $d\log L_g = \frac{dL_g^N}{L_g^N} + \frac{\pi_g^I}{\pi_g^N}dI$ for g={U, S}, and simplify:

$$d\log L = s_U d\log L_U^N + s_U \frac{\pi_U^I}{\pi_U^N} dI + s_S d\log L_S^N + s_S \frac{pi_S^I}{\pi_S^N} dI$$

$$\Leftrightarrow d\log L = \underbrace{\left(s_U \frac{\pi_U^I}{\pi_U^N} + s_S \frac{\pi_S^I}{\pi_S^N} \right)}_{=\Pi} dI + s_U d\log L_U^N + s_S d\log L_S^N \tag{B.17}$$

Next, we turn to totally differentiate the labour demand function, given by eq. (B.9):

$$\begin{aligned} d\log w_g &= \alpha\left(d\log K - d\log L\right) + (\beta-1)\left(d\log L_g - d\log L\right) \\ &= \alpha\left(-\frac{\alpha-1}{1-\alpha+\lambda} d\log L - d\log L\right) + (\beta-1)\left(d\log L_g - d\log L\right) \\ &= \alpha\underbrace{\left(-\frac{\alpha-1}{1-\alpha+\lambda} - 1\right)}_{\frac{1-\alpha-1+\alpha-\lambda}{1-\alpha+\lambda} = -\frac{\lambda}{1-\alpha+\lambda}} d\log L + (\beta-1)\left(d\log L_g - d\log L\right) \\ &= \underbrace{-\frac{\alpha\lambda}{1-\alpha+\lambda}}_{=\varphi} d\log L + (\beta-1)\left(d\log L_g - d\log L\right) \end{aligned} \tag{B.18}$$

In the second row, we substitute for $d\log K$ the expression derived in eq. (B.14). Now, we plug in (B.15) and (B.17), and solve for $d\log L_g^N$.

$$\begin{aligned} d\log w_g =& \varphi\left[\Pi dI + s_g d\log L_g^N + s_{g'} d\log L_{g'}^N\right] \\ &+ (\beta-1)\left(\left[\frac{\pi_g^I}{\pi_g^N} dI + d\log L_g^N\right] - \left[\Pi dI + s_U d\log L_g^N + s_{g'} d\log L_{g'}^N\right]\right) \\ &= \varphi \Pi dI - (\beta-1)\Pi dI + (\beta-1)\frac{\pi_g^I}{\pi_g^N} dI \\ &+ \varphi s_{g'} d\log L_{g'}^N - (\beta-1) s_{g'} d\log L_{g'}^N \\ &+ \varphi s_g d\log L_g^N + (\beta-1) d\log L_g^N - (\beta-1) S_g d\log L_g^N \end{aligned} \tag{B.19}$$

where the last three rows each contain all terms related to one of the three key variables. This can be simplified to yield:

$$
\begin{aligned}
d\log L_g^N\left[\varphi s_g + (\beta-1)(1-s_g)\right] =& d\log w_g - \left((\varphi-(\beta-1))\,\Pi + (\beta-1)\,\frac{\pi_g^I}{\pi_g^N}\right) dI \\
& - (\varphi-(\beta-1))\, s_{g'}\, d\log L_{g'} \\
=& \frac{1}{\varphi s_g + (\beta-1)(1-s_g)}\, d\log w_g \\
& - \frac{(\varphi-(\beta-1))\,\Pi + (\beta-1)\,\frac{\pi_g^I}{\pi_g^N}}{\varphi s_g + (\beta-1)(1-s_g)}\, dI \\
& - \frac{(\varphi-(\beta-1))\, s_{g'}}{\varphi s_g + (\beta-1)(1-s_g)}\, d\log L_{g'}
\end{aligned}
\tag{B.20}
$$

This is equation A.7 from the Online Appendix of DSS. It is a function that depends on the own wage, the immigrant shock, and the labour supply of the other skill group, which itself depends on its own wage, the immigrant shock, and the labour supply of this skill group. To solve this means to derive an equation that depends only on wages (or employment) and the immigrant shock. Therefore, we now replace g by U and g' by S, and insert the expression for S into the corresponding expression for U:

$$
\begin{aligned}
d\log L_U^N =& \frac{1}{\varphi s_U + (\beta-1)\underbrace{(1-s_U)}_{s_S}}\, d\log w_U - \frac{(\varphi-(\beta-1))\,\Pi + (\beta-1)\,\frac{\pi_U^I}{\pi_U^N}}{\varphi s_U + (\beta-1)\underbrace{(1-s_U)}_{=s_S}}\, dI \\
& - \frac{(\varphi-(\beta-1))\, s_S}{\varphi s_U + (\beta-1)\underbrace{(1-s_U)}_{=s_S}} \Bigg[\frac{1}{\varphi s_S + (\beta-1)\underbrace{(1-s_S)}_{=s_U}}\, d\log w_S \\
& - \frac{(\varphi-(\beta-1))\,\Pi + (\beta-1)\,\frac{\pi_S^I}{\pi_S^N}}{\varphi s_S + (\beta-1)\underbrace{(1-s_S)}_{=s_U}}\, dI - \frac{(\varphi-(\beta-1))\, s_S}{\varphi s_U + (\beta-1)\underbrace{(1-s_U)}_{=s_S}}\, d\log L_U^N \Bigg]
\end{aligned}
\tag{B.21}
$$

With all $1-s_g$ replaced by $s_{g'}$, we next collect all $d\log L_U^N$ terms on the LHS:

$$
\begin{aligned}
& d\log L_U^N - \frac{\varphi s_S - (\beta-1)\, s_S}{\varphi s_U + (\beta-1)\, s_S}\, \frac{\varphi s_U - (\beta-1)\, s_U}{\varphi s_S + (\beta-1)\, s_U}\, d\log L_U^N \\
=& \frac{1}{\varphi s_U + (\beta-1)\, s_S}\, d\log w_U - \frac{\varphi s_S - (\beta-1)\, s_S}{\varphi s_U + (\beta-1)\, s_S}\, \frac{1}{\varphi s_S + (\beta-1)\, s_U}\, d\log w_S \\
& + \frac{\varphi s_S - (\beta-1)\, s_S}{\varphi s_U + (\beta-1)\, s_S}\, \frac{(\varphi-(\beta-1))\,\Pi + (\beta-1)\,\frac{\pi_S^I}{\pi_S^N}}{\varphi s_S + (\beta-1)\, s_U}\, dI \\
& - \frac{(\varphi s_S - (\beta-1))\,\Pi + (\beta-1)\,\frac{\pi_U^I}{\pi_U^N}}{\varphi s_U + (\beta-1)\, s_S}\, dI
\end{aligned}
\tag{B.22}
$$

Now, multiply by $\varphi s_U + (\beta-1)\,s_S$ and simplify a little bit:

$$
\begin{aligned}
d\log L_U^N & \left(\varphi s_U + (\beta-1)\,s_S - \frac{(\varphi s_S - (\beta-1)\,s_S)\,(\varphi s_U - (\beta-1)\,s_U)}{\varphi s_S + (\beta-1)\,s_U}\right) \\
&= d\log w_U - \frac{\varphi s_S - (\beta-1)\,s_S}{\varphi s_S + (\beta-1)\,s_U} d\log w_S \\
&+ \frac{\varphi s_S - (\beta-1)\,s_S}{\varphi s_S + (\beta-1)\,s_U}\left[(\varphi-(\beta-1))\,\Pi + (\beta-1)\,\frac{\pi_S^I}{\pi_S^N}\right] dI \\
&- \left[(\varphi s_S - (\beta-1))\,\Pi + (\beta-1)\,\frac{\pi_U^I}{\pi_U^N}\right] dI \\
\Leftrightarrow d\log L_U^N & \left((\varphi s_U + (\beta-1)\,s_S)\,(\varphi s_S + (\beta-1)\,s_U) - (\varphi s_S - (\beta-1)\,s_S)\,(\varphi s_U - (\beta-1)\,s_U)\right) \\
&= (\varphi s_S + (\beta-1)\,s_U)\,d\log w_U - (\varphi s_S - (\beta-1)\,s_S)\,d\log w_S \\
&+ (\varphi s_S - (\beta-1)\,s_S)\left[(\varphi-(\beta-1))\,\Pi + (\beta-1)\,\frac{\pi_S^I}{\pi_S^N}\right] dI \\
&- (\varphi s_S + (\beta-1)\,s_U)\left[(\varphi-(\beta-1))\,\Pi + (\beta-1)\,\frac{\pi_U^I}{\pi_U^N}\right] dI
\end{aligned} \tag{B.23}
$$

To simplify this expression, we have to work on the coefficient pre-multiplying $d\log L_U^N$ and

dI. We begin with dI, i.e., the last two rows:

$$
\begin{aligned}
&[(\varphi s_S-(\beta-1)\,s_S)\,(\varphi-(\beta-1))\,\Pi-(\varphi s_S+(\beta-1)\,s_U)\,(\varphi-(\beta-1))\,\Pi] \\
&+\left[(\varphi s_S-(\beta-1)\,s_S)\,(\beta-1)\,\frac{\pi_S^I}{\pi_S^N}-\left(\varphi s_S-(\beta-1)\,\underbrace{s_U}_{1-s_S}\right)(\beta-1)\,\frac{\pi_U^I}{\pi_U^N}\right]dI \\
&=\left[(\varphi-(\beta-1))\,\Pi\left(\underbrace{\varphi s_S-\varphi s_S}_{=0}-(\beta-1)\,s_S-(\beta-1)\,s_U\right)\right] \\
&+\left[(\varphi s_S-(\beta-1)\,s_S)\,(\beta-1)\,\frac{\pi_S^I}{\pi_S^N}-(\varphi s_S-(\beta-1)\,s_S+(\beta-1))\,(\beta-1)\,\frac{\pi_U^I}{\pi_U^N}\right]dI \\
&=\left[-(\varphi-(\beta-1))\,\Pi\,(\beta-1)\left(\underbrace{s_S+s_U}_{=1}\right)\right] \\
&+\left[(\varphi s_S-(\beta-1)\,s_S)\,(\beta-1)\,\frac{\pi_S^I}{\pi_S^N}-(\varphi s_S-(\beta-1)\,s_S\,(\beta-1))\,\frac{\pi_U^I}{\pi_U^N}-(\beta-1)^2\,\frac{\pi_U^I}{\pi_U^N}\right]dI \\
&=[-(\varphi-(\beta-1))\,\Pi\,(\beta-1)] \\
&+\left[(\varphi s_S-(\beta-1)\,s_S)\,(\beta-1)\left(\frac{\pi_S^I}{\pi_S^N}-\frac{\pi_U^I}{\pi_U^N}\right)-(\beta-1)^2\,\frac{\pi_U^I}{\pi_U^N}\right]dI \\
&=\left[-(\varphi-(\beta-1))\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}\right)(\beta-1)\right] \\
&+\left[(\varphi s_S-(\beta-1)\,s_S)\,(\beta-1)\left(\frac{\pi_S^I}{\pi_S^N}-\frac{\pi_U^I}{\pi_U^N}\right)-(\beta-1)^2\,\frac{\pi_U^I}{\pi_U^N}\right]dI \\
&=\left[-\varphi\,(\beta-1)\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}\right)+(\beta-1)^2\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}\right)-(\beta-1)^2\,\frac{\pi_U^I}{\pi_U^N}\right. \\
&\quad\left.+\,(\varphi-(\beta-1))\,s_S\,(\beta-1)\left(\frac{\pi_S^I}{\pi_S^N}-\frac{\pi_U^I}{\pi_U^N}\right)\right]dI \\
&=\left[-\varphi\,(\beta-1)\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}\right)+(\varphi\,(\beta-1))\left(s_S\frac{\pi_S^I}{\pi_S^N}-s_S\frac{\pi_U^I}{\pi_U^N}\right)\right. \\
&\quad\left.-\,(\beta-1)^2\left(s_S\frac{\pi_S^I}{\pi_S^N}-s_S\frac{\pi_U^I}{\pi_U^N}\right)+(\beta-1)^2\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}-\frac{\pi_U^I}{\pi_U^N}\right)\right]dI \\
&=\left[-\varphi\,(\beta-1)\left(\underbrace{s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}-s_S\frac{\pi_S^I}{\pi_S^N}+s_S\frac{\pi_U^I}{\pi_U^N}}_{=(s_U+s_S)\frac{\pi_U^I}{\pi_U^N}=\frac{\pi_U^I}{\pi_U^N}}\right)\right. \\
&\quad\left.+\,(\beta-1)^2\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}-\frac{\pi_U^I}{\pi_U^N}-s_S\frac{\pi_S^I}{\pi_S^N}+s_S\frac{\pi_U^I}{\pi_U^N}\right)\right]dI \\
&=\left[-\varphi\,(\beta-1)\,\frac{\pi_U^I}{\pi_U^N}+(\beta-1)^2\left(\underbrace{\frac{\pi_U^I}{\pi_U^N}-\frac{\pi_U^I}{\pi_U^N}}_{=0}\right)\right]dI \\
&=-\varphi\,(\beta-1)\,\frac{\pi_U^I}{\pi_U^N}dI
\end{aligned}
\tag{B.24}
$$

Now, let us consider the coefficient pre-multiplying the LHS:

$$\begin{aligned}
&((\varphi s_U+(\beta-1)s_S)(\varphi s_S+(\beta-1)s_U)-(\varphi s_S-(\beta-1)s_S)(\varphi s_U-(\beta-1)s_U))\\
=&\varphi^2 s_U s_S+\varphi s_U(\beta-1)s_U+(\beta-1)s_S\varphi s_S+(\beta-1)^2 s_S s_U\\
&-\varphi^2 s_U s_S+\varphi s_S(\beta-1)s_U+(\beta-1)s_S\varphi s_U-(\beta-1)^2 s_S s_U\\
=&\varphi(\beta-1)s_U^2 s_U+\varphi(\beta-1)s_S^2+2\varphi(\beta-1)s_S s_U\\
=&\varphi(\beta-1)(s_U+s_S)^2\\
=&\varphi(\beta-1)
\end{aligned} \tag{B.25}$$

Inserting expression (B.24) and (B.25) into eq. (B.23) and rearranging terms gives:

$$\begin{aligned}
&\varphi(\beta-1)\,d\log L_U^N\\
&=(\varphi s_S+(\beta-1)s_U)\,d\log w_U-(\varphi s_S-(\beta-1)s_S)\,d\log w_S-\varphi(\beta-1)\frac{\pi_U^I}{\pi_U^N}dI\\
\Leftrightarrow\quad &d\log L_U^N=\frac{(\varphi s_S+(\beta-1)s_U)}{\varphi(\beta-1)}d\log w_U-\frac{(\varphi s_S-(\beta-1)s_S)}{\varphi(\beta-1)}d\log w_S-\frac{\pi_U^I}{\pi_U^N}dI
\end{aligned} \tag{B.26}$$

Finally, to arrive at key equation (2) of the main text of DSS, we only need to divide by dI. The final expression gives the response of native labour to an immigrant-induced change in labour supply as a function of the own wage, the other skill group's wage, and the ratio of skill intensities of immigrants and natives. For $\beta < 1$, the first term on the RHS is unambiguously negative, i.e., employment and wages of a given skill group move in opposite directions, whereas the second term might be positive or negative, depending on the slope of the aggregate demand curve, φ. This slope depends on the capital share in output, α and the degree of capital mobility, λ. The last term shows that a positive immigrant shock will reduce native employment, and this reduction is more negative, the more unskilled immigrants are relative to natives.

B.5 Shift Share Instruments for the German Labour Market

The shift share instrument is widely used in the spatial correlation literature on immigration to deal with the endogeneity of the regional settlement of newly arriving immigrants. In what follows, we describe the implementation of two versions of the shift share instrument, both motivated by the idea that newly arriving immigrants tend to settle in regions in which other immigrants already settled earlier, and that the settlement decision of earlier immigrants is uncorrelated with current demand shocks. While the simple version of the instrument only considers the pattern of the overall immigrant settlement (see, e.g., Altonji and Card 1991), the more complex version further takes into account the source countries in the construction of the instrument (see, e.g., Card, 2001, 2007, 2009; Glitz, 2012; Smith, 2012; Peri and Sparber, 2009; Dustmann and Glitz, 2015).

In the simple version of the shift share instrument, we predict the immigrant inflow into a local labour market based on the foreign population density in some initial period to instrument the actual region level changes in immigrant population shares. Formally,

$$\Delta \tilde{I}^{Pop}_{r,88-93} = \gamma_{r,t_0} \frac{I^{Pop}_{93} - I^{Pop}_{88}}{(N^{Pop}_{r,88} + I^{Pop}_{r,88})} \tag{B.27}$$

where $\gamma_{r,t_0} = I^{Pop}_{r,t_0} / I^{Pop}_{t_0}$ denotes the share of foreigners in the population that resides in region r in some initial period t_0, $I^{Pop}_{93} - I^{Pop}_{88}$ is the nationwide net inflow of immigrants between 1988 and 1993, and $Pop_{r,88}$ is the population in region r in 1988. The initial year t_0 in our example is 1961, the earliest year for which population data for natives and immigrants is available. We use census information from the GESIS data archive (available online under the file name ZA2472) and combine this data with population data from the Federal Statistical Office for years 1985-2001, which we reassembled from Statistical Yearbooks and published tables (both available online). To obtain first stage results, we regress the changes in local immigrant employment shares in 1988-1993 on the instrument $\Delta \tilde{I}^{Pop}_{r,88-93}$. The results in Table 3.8 (see last rows) suggest that the historical immigrant settlement pattern is indeed a strong predictor for settlement of immigrants between 1988 and 1993, with F-statistics of 30.24 to 32.56 (depending on the measure for the outcome variable). However, we do not use this instrument in our main analysis because it is only available for 112 out of 204 local labour markets.

A more sophisticated version of the shift share instrument relies, in principle, on a similar idea, but instead of using the overall share of past immigrant settlements, it uses information on the source country of arriving immigrants to account for more detailed ties between specific ethnic enclaves. Formally, the instrument is constructed by interacting past *country-specific* immigrant densities across regions with nationwide *country-specific* net inflows of immigrants,

$$\Delta \tilde{I}^{Emp}_{r,88-93} = \sum_c \lambda_{c,r,t_0} \frac{I^{Pop}_{c,93} - I^{Pop}_{c,88}}{(N^{Emp}_{r,88} + I^{Emp}_{r,88})} \tag{B.28}$$

where $\lambda_{c,r,t_0} = I^{Emp}_{c,r,t_0} / I^{Emp}_{c,t_0}$ denotes the share of all immigrants from source country c that work in region r in some base year, and $I^{Pop}_{c,93} - I^{Pop}_{c,88}$ is the nationwide net inflow of country c immigrants between 1988 and 1993.[4] The denominator scales the predicted net change in levels by total employment (natives+immigrants) in region r in year 1988. We choose 1975 as initial year because it is the earliest year available in our administrative records, and because it comes as close as possible to the settlement structure underlying our distance instrument (although the latter dates back one more decade). The results are summarised in Table B.2.

[4] We use five nationality groups defined as (1) Poland, the Former Soviet Union, Romania, and Central and Eastern Europe, (2) Turkey, (3) Italy, (4) Former Yugoslavia, Greece, and Portugal, and (5) Western Europe plus rest of the world.

Somewhat surprisingly, column 1 (which corresponds to column 5 in Table 3.4) shows a low correlation (F-statistic=3.34) between predicted and actual immigrant employment growth, which disqualifies this instrument for our analysis. The entries in column 2 demonstrate that the performance of the supply push instrument deteriorates further if we (in analogy to DG) extend the observation period backward and forward to 1985-1995.

The relatively poor performance of the shift-share instrument seems to contradict the evidence in a recent study of DG. They use the shift-share instrument to predict changes in the local skill-specific labour supply, reporting F-statistics well above 20 in their analysis of all skill groups. There is, however, an important conceptual differences between the present analysis and the study of DG. Notably, we do not use skill-specific inflows of immigrants (relying on variation in immigration across regions and skill cells) but instead base our analysis on the overall immigrant inflow. In the following, we show that when we estimate a first stage specification exploiting skill-specific variation in employment growth, we obtain very similar results as DG, yet these appear to be driven by native rather than immigrant employment changes.

We begin by defining the change in skill-specific employment between 1985 and 1995 as the percentage change of the total (native+immigrant) employment of skill group i in region r between 1985 and 1995.[5] In concordance with the endogenous variable of the first stage model, we define the shift-share instrument to predict the *skill-specific* inflow of immigrants by interacting the predicted inflow in a region with a nation-wide average skill distribution of immigrants who arrived between 1985 and 1995. Formally,

$$\Delta\tilde{I}^{Emp}_{i,r,85-95} = \frac{\sum_c \lambda_{c,r,t_0}\theta_{c,i}(I^{Pop}_{c,95} - I^{Pop}_{c,85})}{(N^{Emp}_{r,85} + I^{Emp}_{r,85})} \tag{B.29}$$

The three components in the numerator reflect, for each source country c, the initial regional distribution of immigrant employment in 1975 ($\lambda_{c,r,t_0} = I^{Emp}_{c,r,t_0}/I^{Emp}_{c,t_0}$), the average skill distribution between 1985 and 1995, and the total net inflow between 1985 and 1995. The denominator scales the predicted inflow by skill-specific employment in the base year (1985). We use published data from DG to obtain the skill-specific regional immigrants net flows by source country (numerator).[6]

Column 3 in Table B.2 shows the first stage relation between the relative change in skill-specific total employment (natives+immigrants) and the predicted skill-specific inflows of immigrants (conditional on a full set of region and skill group fixed effects). Although our slope parameter is somewhat larger and the F-statistic somewhat smaller compared to DG's results (slope: 0.448 compared to 0.297 (DG); F-statistic: 16.26 compared to 26.0 (DG)), we arrive at substantially similar conclusions: the instrument works well in this setting. However, once we decompose the change in total employment into the change in natives and

[5] Note that, as in DG, we include ethnic Germans and East Germans in this analysis.

[6] DG obtain immigrant net flows by source country from the Federal Statistical Office and the skill distribution from the German Microcensus using information on the year of immigration and the current education level. Note that DG's data distinguished between 15 nationality groups.

immigrant employment, and regress each component (both divided by total employment) separately on the instrument, we find that the predicted immigrant growth between 1985 and 1995 is highly correlated with native employment changes but virtually uncorrelated with changes in immigrant employment (see columns 4 and 5 in Table B.2).[7] This seems to contradict the original enclave-based idea of the supply push instrument, which should lead to a positive correlation between the percentage change in *immigrant* employment and the predicted immigrant growth.

In the following, we suggest a possible explanation for why the complex shift share instrument performs worse than a distance-based measure in the German context: first, the initial settlements, especially of former guest workers, were highly concentrated in a relatively small number of regions, leading to exceptionally high immigrant employment rates in some areas, but still low shares in other, geographically close regions; second, immigrants from former Eastern Bloc states were virtually non-existent introducing substantial randomness in the assignment of later flows. For example, based on 1975 data, we find that there are areas of high concentration in Baden-Wuerttemberg and North Rhine-Westphalia as well as the wider area of Munich and some dispersed areas further north (like Hamburg). In contrast, the north and eastern border of Bavaria reveals rather low concentrations of immigrants. Applying the mechanics of the shift share instrument, we can calculate the counterfactual immigrant density that would be observed if future immigrant settlements were determined only by the initial density in 1975 (see Figure B.2). The shift share instrument — using shares in 1975 — overpredicts changes in immigrant shares between 1985 and 1995 in areas that were initially high immigrant regions. That is, later immigrants did *not* move proportionately into areas that were initially high immigrant regions. This points to some spillover effects, possibly because the former guest workers who were granted the right to stay, required further and cheaper housing space as they started to reunify their families in later decades.[8] In addition, the shift share IV performs particularly bad in many southern regions that either share a border with high immigrant regions as of 1975 or are located further east. This illustrates the inability of the shift share prediction to adequately predict the settlement of Eastern Bloc migrants.

[7] Note that we focus on the percentage change in immigrant employment rather than the total labour force. Moreover, we merge medium- and high-skilled workers in the analysis, which stands in contrast to the analysis in DG. However, we repeated the same exercise for the labor force subdivided into three skill groups and obtained very similar results: while estimates using the percentage change in the total labour force are even closer to DG's results, the F-statistic is close to zero when we use the percentage change in the immigrant labour force. Also note that DG use weights for total labour force (including immigrants) in the *tradable* sector, whereas we present results based on total employment in all sectors, but exclude immigrants. But again, in unreported results, we found that using the alternative weighting scheme does not have a relevant impact on the results and we arrive at the same conclusions.

[8] Anecdotal evidence suggests that housing space was scarce in the hot spots of 1975. As immigrants reunified their families over the subsequent decade, they may have been forced to move to neighbouring regions.

B.6 Appendix Tables

Table B.1: Comparison of Different Estimation Methods

	Wages		Employment	
	Plug-in OLS	Generated Instr. Var.	Plug-in OLS	Generated Instr. Var.
	(1)	(2)	(3)	(4)
Panel A: All skill groups				
2nd stage coefficient	-0.677	-0.677	-1.125	-1.125
- Robust	(0.247)	(0.285)	(0.700)	(0.745)
- No Bootstrap	–	(0.246)	–	(0.698)
- Bootstrap	(0.281)	(0.272)	(0.718)	(0.697)
Panel B: Unskilled				
2nd stage coefficient	-0.695	-0.658	-2.610	-2.593
- Robust	(0.505)	(0.496)	(1.123)	(1.224)
- No Bootstrap	–	(0.478)	–	(1.116)
- Bootstrap	(0.459)	(0.429)	(1.166)	(1.160)
Panel C: Skilled				
2nd stage coefficient	-0.581	-0.586	-0.917	-0.917
- Robust	(0.244)	(0.274)	(0.808)	(0.819)
- No Bootstrap	–	(0.246)	–	(0.807)
- Bootstrap	(0.294)	(0.295)	(0.779)	(0.795)
Local labour markets	204	204	204	204

Note: Table shows the baseline results reported in Table 5 under two different estimation methods and for different computations of standard errors. Columns 1 and 3 refer to our main approach which replicates the DSS estimation, whereas columns 2 and 4 report the Generated Instrumental Variable (GIV) approach. All models include a linear region-specific trend for years 1986-1988. We report up to three sets of standard errors: robust standard errors for both approaches, non-bootstrapped standard errors for the GIV approach, and bootstrapped standard errors again for both approaches.
Source: SIAB 7510

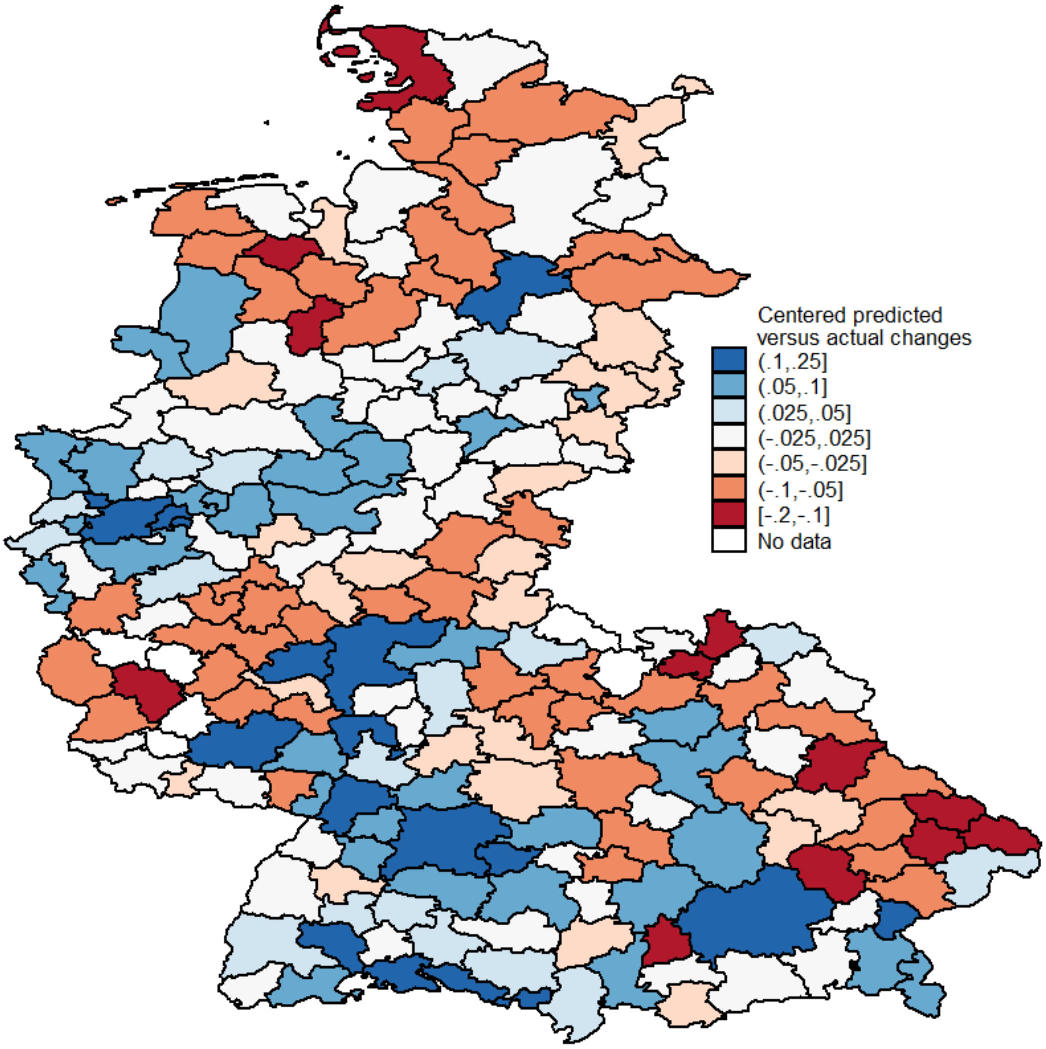
Table B.2: Overview of Shift Share Instruments

	Overall Inflow		Skill-Specific Inflow		
	Immigrant Employment 1988-1993 (col. 5, Tab. 3.4)	Immigrant Employment 1985-1995	Total Employment 1985-1995 (comp. DG)	Native Employment 1985-1995	Immigrant Employment 1985-1995
	(1)	(2)	(3)	(4)	(5)
Predicted inflow using 1975 regional distrib. of immigrants	0.045 (0.025)	0.009 (0.022)	0.448 (0.111)	0.529 (0.125)	-0.081 (0.032)
R-squared (adjusted)	0.027	0.00	0.93	0.92	-0.06
F-statistic (excl. Instrument)	3.34	0.17	16.27	18.02	6.26
Local labour markets	204	204	204	204	204
Observations	204	204	408	408	408

Note: Table summarises the relationship between the predicted immigrant inflow and actual changes in immigrant employment or labour force shares. Column 1 replicates the results of column 5, Table 3.4. Column 2 adjusts the observation period to 1985-1995. In columns 3-5, we use as instrument the predicted skill-specific inflow from DG, showing that the correlation between an immigrant-induced change in relative skill supplies and the local employment is primarily driven by changes in the relative supplies of natives and not immigrants.
Source: SIAB 7510

B.7 Appendix Figures

Figure B.1: Predicted vs. Observed Changes in Immigrant Employment Shares between 1985 and 1995



Note: Figure shows regional differences between the predicted and observed changes in the immigrant employment share between 1985 and 1995. The difference is centred around zero by subtracting from each region the average difference of all regions. The predicted changes are based on a shift share instrument following DG (see Appendix B.5 for details).
Source: SIAB 7510

C Appendix to Chapter 4: Finding Your Right (or Left) Partner to Merge

C.1 Appendix Tables

Table C.1: Description of Variables

Variable	Short Description
Regional Controls	Dummy variables for each municipality/county prior to the merger
Political Controls	Dummies for the party of the local mayor; dummies for the dominant party in the community council; within coalition share of same party mayors
Demographic Controls	Total population in a community; share of population aged 18-29, 29-64, and 65+; ratio of female/male population (gender ratio)
Expenditure Controls	Total expenditures, and expenditure subcategories for: administration, schooling, public order and emergency services, culture, social security, sport, infrastructure, business development subsidies, and public utilities. Expenditure and revenue data is included in the analysis on a per person basis. Each expenditure, budget, and population variable is included in levels and in terms of absolute deviations from the population-weighted average within the coalition
Revenues	Municipal share of the income tax; trade tax, property tax on agricultural land, property tax on developed land, municipal share of the value added tax, and interest on investments

Note: Table summarises the main variables used in the analysis.

Table C.2: Probit Regression Results using Different Measures of Budget Heterogeneity and Budget Composition: Voluntary vs. Forced

	HHI-Index municipal level (1)	HHI-Index coalition level (2)	(2) + budget controls (3)	Alternative Budget Composition (4)
Dominant Party Share	0.4396	0.4409	0.4094	0.3426
(within coalition partners)	(0.2408)	(0.2474)	(0.2492)	(0.2429)
	[0.1318]	[0.1310]	[0.1211]	[0.1016]
Community size	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Δ Community size	0.0002	0.0002	0.0001	0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Total Pop. Involved	-0.0001	-0.0001	-0.0001	-0.0002
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Merger Size	0.0746	0.0360	0.0416	0.0864**
	(0.0374)	(0.0440)	(0.0440)	(0.0376)
HHI of Expenditures	1.1217	0.5739	0.6896	
(municipal level)	(0.5406)	(0.4972)	(0.5115)	
HHI-spread of Expenditures		1.5337*	1.4526	
(coalition level)		(0.8305)	(0.8261)	
Total Expenditures			-0.0003	-0.0004
			(0.0001)	(0.0001)
Rev. Property Tax A			0.0034	0.0111
			(0.0053)	(0.0065)
Rev. Property Tax B			-0.0022	0.0016
			(0.0014)	(0.0020)
Rev. Trade Tax			0.0012**	0.0006
			(0.0005)	(0.0004)
Rev. Interest payments			0.0025	0.0034
			(0.0013)	(0.0015)
Δ Total Expenditures				0.0001
				(0.0001)
Δ Rev. Property Tax A				-0.0119
				(0.0078)
Δ Rev. Property Tax B				-0.0063
				(0.0025)
Δ Rev. Trade Tax				0.0007
				(0.0006)
Δ Rev. Interest payments				-0.0014
				(0.0021)
Fixed Effects				
Regional Dummies	✓	✓	✓	✓
Political Controls	✓	✓	✓	✓
Demographics	✓	✓	✓	✓
Budget Composition	✓	✓	✓	✓
Observations	1,314	1,309	1,309	1,314
Pseudo R-squared	0.2119	0.2207	0.2293	0.2250

Notes: Table shows coefficient estimates of Probit regressions of an indicator of voluntary mergers on the dominant party share in a coalition. A leading Δ indicates variables measured as differences to the coalition mean. The regression constant is not reported. Fiscal and financial variables are measured in per capita values. Standard errors in round parentheses and marginal effects at the mean in square brackets. All standard errors are clustered on the merger level.
Source: Statistical Office of Berlin-Brandenburg

Table C.3: Probit Regression Results using Different Measures of Budget Heterogeneity and Budget Composition: Voluntary vs. Simulated

	HHI-Index municipal level (1)	HHI-Index coalition level (2)	(2) + budget controls (3)	Alternative Budget Composition (4)
Dominant Party Share	0.2480	0.2457	0.2431	0.2114
(within coalition partners)	(0.1221)	(0.1224)	(0.1223)	(0.1230)
	[0.0363]	[0.0362]	[0.0358]	[0.0308]
Community size	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Δ Community size	0.0002	0.0002	0.0002	0.0002
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Total Pop. Involved	-0.0001	-0.0001	-0.0001	-0.0001
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Merger Size	0.0681	0.0676	0.0685	0.0699
	(0.0213)	(0.0243)	(0.0245)	(0.0215)
HHI of Expenditures	0.2673	0.2784	0.3429	
(municipal level)	(0.2068)	(0.1946)	(0.2061)	
HHI-spread of Expenditures		0.0527	0.0327	
(coalition level)		(0.3214)	(0.3217)	
Total Expenditures			-0.0001	-0.0001
			(0.0001)	(0.0001)
Rev. Property Tax A			0.0014	0.0055
			(0.0024)	(0.0033)
Rev. Property Tax B			-0.0002	0.0003
			(0.0008)	(0.0009)
Rev. Trade Tax			0.0003	0.0004
			(0.0002)	(0.0002)
Rev. Interest payments			0.0009	0.0011
			(0.0006)	(0.0007)
Δ Total Expenditures				-0.0000
				(0.0001)
Δ Rev. Property Tax A				-0.0068
				(0.0037)
Δ Rev. Property Tax B				-0.0012
				(0.0013)
Δ Rev. Trade Tax				-0.0001
				(0.0003)
Δ Rev. Interest payments				-0.0002
				(0.0009)
Fixed Effects				
Regional Dummies	✓	✓	✓	✓
Political Controls	✓	✓	✓	✓
Demographics	✓	✓	✓	✓
Budget Composition	✓	✓	✓	✓
Observations	9,825	9,666	9,666	9,825
Pseudo R-squared	0.0702	0.0725	0.0737	0.0725

Notes: Table shows coefficient estimates of Probit regressions of an indicator of voluntary mergers on the dominant party share in the coalition. A leading Δ indicates variables measured as differences to the coalition mean. The regression constant is not reported. Fiscal and financial variables are measured in per capita values. Standard errors in round parentheses and marginal effects at the mean in square brackets. All standard errors are clustered on the merger level.
Source: Statistical Office of Berlin-Brandenburg

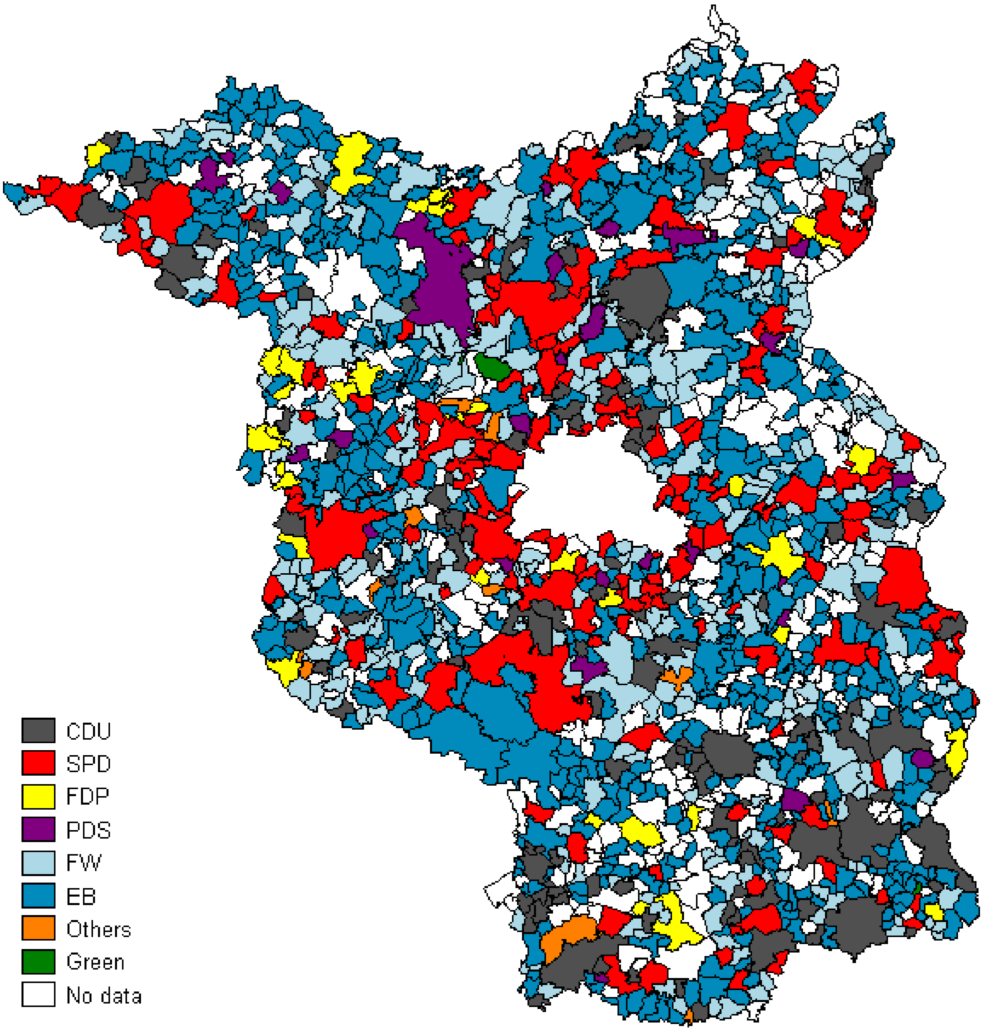
Table C.4: Further Robustness Tests

	Logit Estimation		Merger Size ≥11		Adjusted	Minimum
	Forced	Simulated	Forced	Simulated	Simulation	Population
	(1)	(2)	(3)	(4)	(5)	(6)
Dominant Party Share	0.6324	0.4943	0.4060	0.1915	0.1979	0.2407
(within coalition partners)	(0.4456)	(0.2552)	(0.2451)	(0.1203)	(0.1507)	(0.1227)
	[0.1055]	[0.0343]	[0.1230]	[0.0264]	[0.0373]	[0.0345]
Selected controls						
Community size	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Δ Community size	0.0003	0.0004	0.0002	0.0002	0.0002	0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0000)
Total Pop. Involved	-0.0002	-0.0002	-0.0001	-0.0001	-0.0001	-0.0001
	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Merger Size	0.1400	0.1561	0.1125	0.0454	0.1312	0.0779
	(0.0773)	(0.0525)	(0.0679)	(0.0269)	(0.0314)	(0.0266)
Total Expenditures	-0.0004	-0.0002	-0.0002*	-0.0001	-0.0002	-0.0001
	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Rev. Property Tax A	0.0084	0.0104	0.0009	0.0043	0.0050	0.0052
	(0.0091)	(0.0048)	(0.0047)	(0.0024)	(0.0039)	(0.0024)
Rev. Property Tax B	-0.0009	-0.0014	-0.0001	-0.0007	0.0014	-0.0007
	(0.0022)	(0.0014)	(0.0013)	(0.0007)	(0.0011)	(0.0007)
Rev. Trade Tax	0.0011	0.0006	0.0005	0.0002	0.0008	0.0003
	(0.0008)	(0.0003)	(0.0004)	(0.0001)	(0.0004)	(0.0001)
Rev. Interest payments	0.0014	0.0008	0.0007	0.0004	0.0010	0.0004
	(0.0019)	(0.0010)	(0.0010)	(0.0005)	(0.0007)	(0.0005)
(Max-Min) Total Expend.	-0.0002	0.0001	-0.0001	0.0000	0.0001	0.0000
	(0.0003)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
(Max-Min) Rev. Prop. Tax A	-0.0022	-0.0163	-0.0019	-0.0035	-0.0145	-0.0081
	(0.0125)	(0.0089)	(0.0078)	(0.0037)	(0.0049)	(0.0041)
(Max-Min) Rev. Prop. Tax B	-0.0060*	0.0008	-0.0043	0.0000	-0.0013	0.0002
	(0.0033)	(0.0024)	(0.0021)	(0.0013)	(0.0015)	(0.0012)
(Max-Min) Trade Tax	0.0012	0.0000	0.0006	0.0001	-0.0001	-0.0000
	(0.0006)	(0.0004)	(0.0003)	(0.0002)	(0.0003)	(0.0002)
(Max-Min) Rev. Int. payments	0.0045	0.0020	0.0027	0.0013	0.0003	0.0010
	(0.0035)	(0.0014)	(0.0019)	(0.0008)	(0.0010)	(0.0008)
Fixed Effects						
Regional Dummies	✓	✓	✓	✓	✓	✓
Political Controls	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓
Budget Composition	✓	✓	✓	✓	✓	✓
Observations	1,314	9,825	1,152	9,118	4,208	9,825
Pseudo R-squared	0.2279	0.0776	0.2150	0.0736	0.4221	0.0770

Notes: Table shows coefficient estimates of regressing an indicator of voluntary mergers on the dominant party share in the coalition. A leading Δ indicates variables measured as differences to the coalition mean. The regression constant is not reported. Fiscal and financial variables are measured in per capita values. Standard errors in round parentheses and marginal effects at the mean in square brackets. All standard errors are clustered on the merger level.
Source: Statistical Office of Berlin-Brandenburg

C.2 Appendix Figures

Figure C.1: Spatial Distribution of Party Affiliation of Mayors



Note: Figure shows the spatial distribution of the party affiliation of mayors across municipalities in the state of Brandenburg.
Source: Statistical Office of Berlin-Brandenburg

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Selbstständigkeitserklärung

Ich erkläre, dass ich die vorliegende Arbeit selbstständig und nur unter Verwendung der angegebenen Literatur und Hilfsmittel angefertigt habe.

Ich bezeuge durch meine Unterschrift, dass meine Angaben über die bei der Abfassung meiner Dissertation benutzten Hilfsmittel, über die mir zuteil gewordene Hilfe sowie über frühere Begutachtungen meiner Dissertation in jeder Hinsicht der Wahrheit entsprechen.

Berlin, den

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